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THE CONCEPT OF PURPOSE IN BIOLOGY

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1. INTRODUCTION

THE propriety and utility of the concept of purpose in biology is such a perennial subject of discussion that some justification for adding to the literature on the matter seems required. This justification is partly the importance of the matter, and partly a belief that there are certain aspects of the problem which have not received adequate attention in these discussions. Briefly, these concern, firstly, the fact that a living organism is not a single agent, but a nexus (both spatial and temporal) of agents; and secondly, the distinction between short range purpose as applied to a single act of such an agent, and long range purpose as applied to the end to which a series of acts is directed.

The importance which a biologist ascribes to the problem of purpose must be influenced by his special interests. There are great tracts of biological research where the problem of purpose is as irrelevant as quantum physics for engineering. But there are other regions of biology where it is of the first importance. For instance, the biologist may aspire to understand the behavior of an amoeba or of a developing egg in the same sense (even though much less completely) as he understands the behavior of his fellow man or his dog. No amount of knowledge of the chemistry and physics of this behavior would give this kind of understanding. At most, such a knowledge would tell him that the chemical and physical processes underlie the macroscopic behavior by which he judges that the man or the dog is acting purposefully.

Again, one of the special interests of biology is to arrive at an understanding of the process by which the higher organisms have evolved from the lower, and since we have primary self-knowledge that one of the higher organisms can act purposefully, to deny purpose in the lower organisms must involve a radical breach of continuity somewhere between the lower organism and man. Hence an adequate theory of evolution cannot be formulated without forming a judgment as to whether or not purpose is characteristic of all living organisms.

Some scientists and philosophers regard the purposefulness of which we are intuitively aware in ourselves as having

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no causal significance. To them it is simply the mental correlate for a physical process, to which alone causal efficacy is to be ascribed. This question will be discussed in Sections 6 and 8; in the meantime we will assume the causal efficacy of purpose where it occurs.

Purposive interpretation of the responses of organisms (including, for instance, the responses of embryonic or tissue cells) is open to the charge of anthropomorphism. But if we regard man as a part of nature, and not as a unique being observing nature from the outside, we cannot regard anthropomorphic explanations as wrong in principle, though they may be erroneous, or useless, in specific cases.

Two uses of anthropomorphic explanations are inadmissible. The first is when such an explanation appears to be false in fact. For example, if one were to say that the purpose of an egg is to develop into the adult, the statement would almost certainly be false, having regard to the complex nervous apparatus associated with long range purposes in man, and to the considerations developed in Section 4. The other inadmissible use of an anthropomorphic explanation is to use it as a substitute for physical or physiological research. If one states that the cells of the amphibian blastula respond purposively to the stimulus of the evocator (a statement which we will maintain to be true) this must not be used as a substitute for research into the nature of the evocator substance, the changes taking place at the surface of the receptor cell, the physical relation of this to the subsequent reactions of the cell, and so on. But this applies to human purposeful activities also. The recognition that a man's act is purposeful is not a substitute for examining the neural and other processes involved in the act. And conversely, a description of these processes should not be used as a substitute for the conclusion that the act was purposeful.

In this paper I am not setting out to *prove* that purpose is characteristic of vital acts in general. No proof seems conceivable in the sense that other types of explanation would be excluded. As Woodger says (1929, p. 485) "It is always possible to defend microscopic mechanism *in principle*, if any one wishes to do so, by making your mechanism complicated enough, and by 'postulating' enough submechanisms to meet all contingencies. It *cannot* then be refuted, but neither can it be verified." And other types of explanation besides mechanism are in the field, for instance, organismic or Gestalt theories, which we will discuss later. But in this paper, I am starting out from the premise that we have first hand knowledge of the purposefulness of our own conscious behavior, and that it is good scientific method to seek to interpret the acts of other living organisms in terms of the acts of the organism we know best, unless there is good reason to suppose that they are radically different.

We will start, therefore, with the assumption that it is reasonable to suppose that the acts of living organisms, or living constituents of organisms (for example, cells) are directed towards ends, and discuss how far this is consonant with observation; what are the ends to which the acts are directed; what are the agents which are acting thus; and finally we will come to grips with a theory of causation which includes purpose as a causally efficacious factor.

In this discussion I will use the word *purposeful* for an act directed towards an end or goal when that end is present in consciousness, and *purposive* for an act directed towards an end when we have to leave undecided the question whether the end is present in any consciousness or not.

If the argument of this paper is correct, responses of tissue cells to stimuli are purposive acts, though we may not even be aware that these are going on. But the man is not the agent concerned in the response of a tissue cell—the agents are the tissue cells themselves, and we have no adequate grounds either for ascribing, or refusing to ascribe, consciousness to tissue cells. A purposive act, in the sense we are using it, is always the act of an agent, and therefore, in considering whether an act is purposive or not, the first task is to identify the agent concerned. The word *agent* will be defined and characterized in Sections 5 and 7; at present we can use it in an obvious sense.

The philosophical aspect of the discussion which follows has been profoundly influenced by Whitehead's Philosophy of Organism (1929), as I understand it.

2. SOME GENERAL FEATURES OF PURPOSIVE ACTS

This section deals with the macroscopic rather than the microscopic feature of purposive acts (molar, as opposed to molecular, behavior, in Tolman's terminology).

There is no doubt that a very wide range of vital acts, from the development of an egg to complex instinctive behavior, bears at least a superficial resemblance to our own consciously purposeful acts. Before discussing this resemblance and what it implies there is, however, one point of difference to be disposed of.

Our own purposeful behavior is directed towards an end in view which is dependent upon past experiences. The goal is imaged in terms of memory of past acts and their results. In the case of the development of an egg, or a spider spinning its web for the first time, whether there is any image of the future action or not, there can be no image depending upon the memory of past experience. In ourselves, a disposition to act in a certain way in a certain situation seems to be overwhelmingly formed by past experience. But we have to accept the fact that such a disposition can also be formed by the processes of embryonic development. Consider this simple example. Some animals instinctively seek the light, others instinctively avoid it—that is to say, the constitution of the organism as developed from the egg is such that it has the disposition to act in this way. Now it is easy to take an animal which by nature avoids the light, and by giving it an electric shock whenever it runs out of the light into the shade, give it a disposition to avoid darkness and seek the light. The behavior of this animal is now the same as that of another species of animal which is by nature positively phototactic. Placed in the dark, it comes out into the light, and if forcibly kept in the dark will exhibit the same objective signs of fear and striving that it previously did when forcibly kept in the light. It seems that one must accept the fact that there is no essential difference between innate dispositions to specific action in specific situations which have been formed by the ordinary processes of morphogenesis, and those which have been acquired by the modification of innate dispositions by experience. The fact that apparently identical behavior dispositions can be produced by such different processes as morphogenesis and learning seems quite mysterious on current ideas regarding both these processes, and ought to furnish a clue to a deeper understanding of them. This, however, is not directly our present concern, and I will use the term *action disposition* for that state of an organism which results in its responding in a specific way to a stimulus, irrespective of whether this state is the result of morphogenetic

processes alone, or of these modified by relevant experience.

The objective grounds for ascribing purposiveness to vital acts are familiar, though not always acceptable, to everyone. Firstly, such acts typically have results useful to the performer, and this useful end is reached by a series of acts which cannot of themselves be held to be of any use except as stages in the process of reaching the final useful end. The various motions gone through by a bird building its nest are quite meaningless except in relation to the final result.

The mere fact that the activities of living organisms end in results useful to the organism does not itself establish that they are purposive. This utility is capable of explanation, in principle at any rate, on familiar Darwinian lines. Granted that organisms vary in regard to their innate modes of response to stimuli, and that these variations are inherited, then the principle of Natural Selection accounts for the evolution of different types of organisms characterized by activities tending to the survival of these organisms. But though Natural Selection may be invoked to explain how it comes about that the activities of organisms have useful results, the question whether these activities are purposive or not is more fundamental. In fact, what selection does in this respect is to preserve those organisms whose acts are purposed to useful ends, and to eliminate those whose acts are purposed to harmful ends. The utility of the acts of living organisms is therefore not in itself sufficient to establish their purposiveness: rather, it constitutes a reason for examining the acts more closely to discover whether there are other grounds for concluding that they are purposive. One of these other grounds is the familiar fact that the sequence of acts by which the goal is attained is not always the same. On different occasions the organism reaches the same end by different routes. (By the "same" end is meant a "functionally equivalent" end). Even in the most rigidly instinctive behavior the animal has always to fit the details of its action to the special situation. The completed nest, the spider's web, the act of mating, is attained by a train of acts different in detail on every occasion. In embryonic development this is also the case. The most conspicuous example of this is the development of a functional adult, or organ, after the experimental introduction of profound disturbance in the early stages of development. But this is only an extreme case. As every embryologist knows, the course of normal development varies in detail, as does the performance of an instinctive action.

In the sphere of behavior this characteristic is exhibited as *trial and error* when elements of complete novelty are present in the situation, and by *insight* when it is a question of combining into a unitary action existing potentialities of response to elements in the situation which are not normally combined.

Many attempts have been made to eliminate the concept of purpose from the explanation of the fact that different routes may be followed to achieve the same end result. Perhaps the favorite type of theory at present is one which accounts for the facts on the principle used in Gestalt psychology; namely, that certain physical systems tend to a state where the potential energy is at a minimum. It is easy to imagine, or construct, such physical systems which will reach the same end state by intervening stages which differ according to differences in the initial configuration of the system. Let us adapt for our purposes the example given by Petermann in his critical discussion of Gestalt theory. If we have five equal floating

magnetized needles with the North Pole of each uppermost, and suspended above them a fixed magnet with the South Pole downwards, the five floating magnets will finally come to rest in a regular pentagon around the fixed magnet as a centre, no matter how they were arranged to begin with. Variations in the initial arrangements will affect the routes by which they attain their positions in the pentagon, but will not affect the shape and size of the final figure.

3. THE PURPOSIVE AGENTS

In this discussion I am conceiving purpose as characterizing the acts of agents; and this brings us to a source of confusion in the use of the concept of purpose in biology.

It is common, for instance, to distinguish between a reflex and an instinctive act on the grounds that the former is mechanistic, or at least not purposive, while the instinctive act is purposive. This distinction, however, surely involves a confusion as to the agents which carry out the two actions. Let us consider the reflex act of blinking when a speck of dust falls on the cornea, and the instinctive action, culminating in mating, which is the expression of the sexual instinct. To say that the act of blinking is unpurposed, and the behavior ending in mating is purposed, is to imply a comparison between incomparables. The reflex act is composed of a series of cell responses to stimuli—that is to say, the responses of a number of agents—the receptor neurone ending on the cornea, the motor neurone, the muscle cells, and perhaps others. This series of acts is to be compared, not with the acts of the mating animal, but with the entire series of acts composing the life cycle—namely, mating, the fusion of the ovum and spermatozoon, the development of the embryo, and its growth into the adult. To the external observer the series of actions making up the blinking reflex has the useful result of protecting the cornea, and so preserving the power of vision; the series of actions making up the life cycle has the useful result of continuing the species. But there is no agent that we know of which is purposing the preservation of the eye, nor of the continuance of the species. Purpose has to be looked for in the living agents which carry out each individual act of each series. The man is not the agent concerned in the reflex blink, nor is the species the agent concerned in mating, though the interests of each are served by the actions.

In each case, the purpose of the agent—whether it be the cells of the reflex arc, or the animal acting under the sexual urge—is to fulfill its nature (a phrase I will try to make more precise later). The animal has a disposition, an instinct, to act in a certain way in a particular situation. Its goal is to achieve mating. It has no concern with the wider "purpose" of ensuring the continuance of the species.

Consider again the agents in the reflex action, for example, the receptor neurone ending in the cornea. This may also be considered as having a disposition, an instinct, to act in a certain way in the presence of a specific stimulus—in this case, the irritation of a speck of dust lying on the cornea. Here again, the cell is no more concerned with conserving the visual function of the eye than the mating animal was concerned with the continuance of the species. Its goal is a much more modest one. It is simply to fulfill its nature by initiating a nerve impulse. Naturally none of the objective signs of purposiveness are to be observed in this act, for it is inaccessible to the necessary kind of observation. Moreover, even if it were accessible, it is doubtful if these signs would be forthcoming. For the extreme

simplicity of the act gives little scope for variation in the method of accomplishing it, or necessity for indirect methods of achieving the goal. Consequently, whether or not we are to attribute purpose to the neurone, in the same sense as we ascribe it to the mating animal, will depend upon indirect considerations which it is the object of this paper to develop.

4. HORMIC AND BIOLOGICAL GOALS

In order to distinguish between the vague, and surely unjustifiable, use of the word purpose in such a phrase "the purpose of the sexual instinct is the continuance of the species," and the precise and justifiable statement "the purpose of the animal under the sexual urge is to mate", we may perhaps describe mating as the *hormic goal* of sexual activity; there is an agent striving to that end. The continuance of the species, on the other hand, may be called the *biological goal* of sexual activity, because although sexual activity serves that end, we can identify no agent striving towards that end. There is no agent that achieves satisfaction by attaining the biological goal—or, at least, no agent that science at present knows anything about.

The present discussion is concerned only indirectly with biological goals. We are not concerned with how it comes about that the acts of an agent, or of a number of different agents collectively, form a series which has results of value to some organism other than the agents concerned. One may remark, however, that our present knowledge seems to leave us the alternative of an agent, unknown to science, to which the biological goal is a hormic goal; or Natural Selection of undirected variation of hormic goals which has brought about the survival of organisms whose hormic goals had these remote consequences.

Hormic goals themselves require a further classification into *immediate* and *terminal*. Consider the classical case of a wasp such as *Ammophila* which excavates a nest, then goes in search of a caterpillar. Having found one, it paralyzes it with its sting, and takes it to the nest. It then goes in search of another, and eventually, after the nest is sufficiently stocked, lays an egg in it and seals up the nest. It seems unnecessary to suppose that the hormic goal to which the first acts of this series are directed includes the entire series of acts ending in the stocked and sealed nest. It is enough to suppose that the hormic goal is always a little ahead of the present action. The hormic goal at the moment (stinging the caterpillar, putting it in the nest, and so forth) is the immediate hormic goal. The completed, sealed nest is the terminal hormic goal.

To assume that the achievement of, or approach to, one immediate hormic goal arouses another which carries the action further along towards the terminal goal which was not the hormic goal at the start of the action, is not to evade a difficulty. For this is a constant feature of our own experience. In many of our own consciously purposeful actions we have no clear idea at the outset what the final result of the whole business is to be. If we start to follow up a train of thought, we may arrive at a conclusion which is correct, but was quite unforeseen at the beginning, and which indeed may be very surprising to us. If we are unwilling to ascribe to the wasp at the beginning of its action, the purpose of carrying it all through to the completion of the nest, there is nothing more nor less mysterious in the action eventuating in the completed nest, than in our train of thought eventuating in an unforeseen conclusion. In both cases we have to accept the fact that the organization of the organism is such that

the achievement of one hormic goal evokes another one appropriate to the furtherance of the action.

The development of the egg has long been regarded by biologists as a test case for the application of physical as opposed to teleological theories of vital action. But the case against teleology has been exaggerated by confusion of hormic and biological goals. By a false analogy, teleological causation has been held to involve the hypothesis that the egg starts its development with the purpose of becoming an adult, and of "knowing how" to achieve this end. But the adult cannot be the hormic goal of the egg, if only because a considerable part of its development is carried out by relatively independent sub-agents (self-differentiating parts). And it seems certain that the hormic goals of these agents must develop with progressive action as we have described above. We will return to the problem of morphogenesis in a later section.

5. THE NATURE OF THE UNITY OF THE ORGANISM

The conception of the organism as an organization of agents raises the question of the nature of the unity of the organism. Although the question of teleogical causation can be considered apart from this, it is quite fundamental for any understanding of purposive behavior of the type which we actually find in living organisms.

At present, the tendency is to emphasize the unity of the organism as opposed to its multiplicity. And, of course, this unity is a very real and important feature, but should not be allowed to obscure the fact that this unity is a unity of agents which for many purposes must also be considered as individuals.

As discussed more fully later, embryonic development is characterized by the subdivision of the organism into parts, whose relation to the organism as a whole seems to be mainly that of responding to stimuli supplied by the morphogenetic fields, either directly or through the agency of chemical substances localized by these fields. But once these parts, or agents, have responded to these stimuli, they go their own way, often themselves subdividing into sub-agents which respond to the morphogenetic fields of the new parts. The essential independence of these parts is illustrated by the well-known transplantation experiments, where for instance, an amphibian limb-bud transplanted to a foreign site, such as the head, continues to develop as a limb. The relation of self-differentiating parts to the whole is little more than that of parasite to host.

Comparatively late in the development, these relatively independent parts, or agents, become connected up into a new unity by the development of the nervous system. But this new unity does not abolish the individual agents. It is a unity achieved, partly by the formation of a new connection between these agents, partly by the development of a dominant *Central Agent* which supplies stimuli to the sub-agents (effectors) in return for stimuli which other sub-agents (receptors) provide for it. The nervous system is, of course, not the only unifying factor in the adult animal. The hormone system must be reckoned as one of them. The unitary factor found in the developing embryo in the form of morphogenetic fields is also clearly present in the adult; the brain fields of Gestalt psychology, moreover, seem to be of comparable nature (Section 6).

Anatomically, cells are the most striking examples of the sub-agents which are thus mutually related to constitute the organism as a whole. But cells are not

the only, or perhaps even the most characteristic, agents. An agent is any region of activity which acts causally as a unit. This definition logically includes physical objects, an electron for example—and indeed in Whitehead's system the distinction between the living and the non-living is one of degree, not of kind. But now we are considering only living agents —that is to say, regions of activity which exhibit, in their degree, the features by which we commonly judge that organisms as a whole are alive. One obvious criterion is that when isolated from the rest of the organism, they continue for a time at least to manifest the phenomena of life; indeed their life seems to be dependent on their connection with the organism as a whole only in respect to extraneous factors such as supply of nourishment, and so on (as demonstrated by culture of parts of organisms *in vitro*). It is true that this criterion is not always available, but as a rule, it is not difficult to distinguish between parts of an organism which are alive, and parts lower down in the hierarchy of organization, such as atoms, which considered apart from the rest of the organism are not alive.

Cells, then, are only particularly conspicuous examples of agents in this sense. And there are indications that a cell may be a nexus of agents dominated by its own central, coordinating agent. This seems to be the case in most Protozoa, where cell parts such as cilia, and contractile vacuoles, have the same sort of relation to the cell as a whole as the cells of multicellular animals have to the organism as a whole.

On the other hand, we have to recognize agents involving larger regions than cells. Such are the regions carrying the morphogenetic fields, and also what I have called the Central Agent, the agent primarily concerned with the behavior of the organism as a whole. In the higher animals this is situated in the brain, in the sense that this is the place where it receives influences emanating from other agents, mainly through the nervous system, and from which it, in its turn, influences other agents. There is no need to suppose that its limits are closely bound up with any anatomical structure, or even that they are constant. The locus of the Central Agent is the locus of a nexus of processes which is part of a larger spatial and temporal nexus of processes acting and reacting upon each other.

The Central Agent is probably identical with the Ego of Gestalt psychology (Koffka, 1935). This Ego is a field process forming part of a larger field (its behavioral environment), the whole constituting the psycho-physical field. Any part of this field which forms a segregated whole within this larger field is a Gestalt, or agent in the terminology employed in this paper. In Whitehead's terminology, the Central Agent is a nexus of *presiding occasions*.

6. FURTHER CONSIDERATION OF THE NATURE OF PURPOSIVE ACTION

Firstly, what is purposed? Is it, for instance, the action itself, or is it the goal of the action? This would be a false formulation of the problem if by goal we meant the situation produced by the action. The goal of an animal under the sexual urge is not to bring about a state of affairs where mating has taken place, but to mate. The most primitive type of purposive behavior is probably simply to change a present state of discomfort or dissatisfaction into one of well-being or satisfaction. So if we are to draw any distinction between the action and the goal of the action, it must be between the action and the satisfaction which will

accrue from the action. But it is impossible to separate these two—one cannot separate the satisfaction from the means of obtaining the satisfaction. The possible satisfactions of an organism are specific to it, and imply the means of attaining them.

How different organisms came to have these different satisfactions is not part of our present problem—that is a problem of evolution. What is basic to all living organisms, and prior to the evolution of differences between them, is their striving to achieve satisfaction. This applies to the "organism as a whole," whether dominated by a Central Agent or not, and also to its various sub-agents—though the possible satisfactions are very different in the different agents.

Many biologists and psychologists would object to the statement that the purpose of the organism is to achieve satisfaction, and would say that the statement ought to run: It is a property of the organism to act in a certain way in a certain situation, in the same sense as we say that it is the property of oxygen and hydrogen under certain circumstances to combine into water, and that what I have called satisfaction is simply the completion of the process, the arrival at a new stage of equilibrium. The most sustained attempt to establish this view is probably to be found in the Gestalt system of psychology. For this reason, I must discuss the fundamentals of this system very briefly—especially as Köhler has extended Gestalt conceptions to the problem of the regulation of embryonic processes.

The Gestalt conception was originally founded upon consideration of visual perception. It was found that these phenomena displayed characteristics similar to those found in certain physical systems, especially those whose changes are describable on the principle that their potential energy tends to a minimum, as in the example of the floating magnets mentioned above. (The example chosen by Koffka is the tendency of a soap-bubble to assume the spherical shape.) This similarity is considered to be more than an analogy. The field properties of phenomena as experienced are believed to be due to the fact that these phenomena are the correlates of actual physical fields in the brain. Hence the total brain field is called the psycho-physical field. Such a field, or organised part of it, is a Gestalt.

Applied at first to visual perception, it has been developed, especially by Köhler and Koffka, into a complete explanatory system of mental processes and behavior. The Ego for instance, corresponding roughly to my Central Agent, is a special sub-system of the psycho-physical field, to which the rest of the field acts as environment. In his latest book (1935) Koffka says of the psycho-physical field "It is a system of stresses and strains which determines real behavior."

It is fundamental to Gestalt theory that the causal efficacy of the psycho-physical field is to be attributed solely to its physical properties. Although these fields may be the correlates of consciousness, the conscious aspect of the process does not enter into the causal explanations advanced by this theory. (Koffka, p. 65.)

Consciousness of purpose is simply awareness of stresses and strains within the psycho-physical field which are tending to change the total field in a direction towards greater equilibrium. This may be achieved in the case of human purposes by finding the correct idea which will solve an intellectual problem, or by appropriate bodily action where overt behavior is concerned. But the changes in the psycho-physical field which are the

correlates of the correct idea, or which result in the stimulation of the appropriate motor nerves, must be those changes which, under the conditions existing at the moment, will result in a minimum of potential energy in the system as a whole.

Gestalt psychologists, however, find it necessary to have recourse to the principle of *isomorphism* to account for the fact that physical processes in the brain result in behavior which deals successfully with real objects in the external world. According to this principle, there is a real correspondence between phenomena as consciously experienced and the physical processes in the psycho-physical field with which they are correlated; there is also a real correspondence between the processes in the psycho-physical field and external things.

The principle of isomorphism is also used by Gestalt psychologists to meet one of the old difficulties raised against the interpretation of thinking as a mere expression of physical processes in the brain. If the whole of the causal factors in thinking are physical processes in the brain, how is logical thinking possible, and how does it give us true information about the external world? According to Koffka, the intrinsic necessity of a logical conclusion corresponds to the intrinsic necessity of the dynamics of the physical field in the brain.

It is clear, of course, that since thought processes do issue in physical changes in the motor nerves, there must be a physical factor concerned. And indeed there can be no doubt that the general principles of Gestalt psychology are of great value to biologists. But surely the necessity of introducing the principle of isomorphism justifies us in continuing to use the concept of purpose without translating it into physical terms. For if there is a real correspondence between mental and physical processes, it seems just as permissible to give a causal explanation in terms of mental processes as of physical ones.

Let us return now to the processes actually involved in a purposive action, assuming that the end is a causal factor in the process. As we have seen, a purposive action implies both the striving after a specific satisfaction, and the means by which this may be attained. But it is cardinal to the whole idea of purpose that the means are only given in a general way, and have to be adapted to the particular circumstances.

What are the minimum assumptions we must make as to the underlying processes which make possible the adaptation of means to ends? One assumption is that every adaptive act—therefore probably every vital act—is preceded by a process which in the first instance I will call *imagined action*. In the final section we will find a more general formula for this process, which, in the form we are considering at the moment, is a highly developed form of a feature of all vital acts.

The term *imagined action* is adapted from the imagined movements (eingebildete Bewegungen) of Palágyi, who makes it one of the corner-stones of his Naturphilosophie. I use the term imagined action, rather than imagined movement, for various reasons, chiefly because I am including in the term the imagined satisfaction accruing from the movement, for as I have maintained, it is impossible to separate the satisfaction from the means of obtaining the satisfaction. Possibly a better term would be *imagined experience*.

Palágyi is emphatic that imagined movement is not the thought of the movement: it is rather the feeling of the movement. And this I suppose we may accept for our imagined action—it is

something prior either to the action itself or to the thought of the action.

According to Palágyi, every movement (action) is preceded by imagined movement (imagined action). He is referring to the higher organisms where the action is carried out by effectors stimulated by nervous impulse. Here the imagined action corresponds to neurological processes. But, granted that the acts of any organism are directed towards an end, the concept of imagined action seems logically necessary; for the end, even if it is to be achieved by a single muscular or protoplasmic movement, is in the future, and the physical cause of the action must be in the present.

The hormic goals of most purposive actions are not, however, to be achieved by a single act, such as a single muscular contraction, but by a series of acts. In such cases the organism (Central Agent) is experiencing imagined action which persists longer than the duration of a single act. This holds even in such a simple purposive action as movement directed towards an object, for slight deviations from the direct path have constantly to be corrected by subsequent movements. Each individual act is determined by a persistent factor which is the general imagined action and a variable factor which is the perception of the situation at the moment as it has developed from previous acts.

Imagined action is closely connected with perception. Indeed many psychologists would certainly say that it *is* perception. Let us rather say that imagined action is the way in which an object or situation is perceived. It is characteristic of present day psychology to stress the conative aspect of perception. Thus we perceive an orange primarily as something to eat, an angry dog as something to avoid. In its extreme form this is the bodily adjustment theory of perception: according to this theory, perception *is* the precursory adjustment of the body to the appropriate action. Behaviorists consider that this bodily adjustment consists of actual minimal contractions, or at least tensions, of the muscles. It does not seem necessary, however, to go so far as that. One may look upon imagined action as a certain orientation of the processes of the Central Agent. More precision will be given to this phrase in Section 8.

7. MORPHOGENESIS

This section is primarily concerned with showing that embryonic development may be interpreted as of the nature of behavior, especially of instinctive behavior; and therefore, if the previous argument is correct, is purposive. Two main problems confront us here. Firstly, the formation of sub-agents, the nature of their mutual relations and of their responses to stimuli; and secondly, the differentiation and development of the purposes by which their actions—forming in the aggregate the process of morphogenesis—are influenced.

An essential feature of the early stages of embryonic development is the multiplication of agents. The most obvious of these are the cells, but as has already been emphasized, the locus of an agent is not necessarily bound up with any anatomical boundaries. It may be the region carrying a morphogenetic field, for instance, and this may be the whole or a part of the organism. The conception of the sundering of morphogenetic fields into sub-fields is a familiar outcome of the experimental study of morphogenesis—for example, the sundering of a limb-field out of the whole, and its subsequent sundering into smaller fields dominating the development of the parts of the limbs.

In consistence with our general conception of the organism, the carrier of a morphogenetic field is an agent in that it acts causally as a unit. But although it is a unit from this point of view, it is a multiplicity from other points of view. It may contain, or be composed of, subfields which are also agents in that they may act as units within the main field. In this respect a morphogenetic field is comparable with a molecule, which in many respects acts as a unit, though composed of parts which also, under certain circumstances, act as units.

The causal action of these agents has now to be considered. The work of Holtfreter, of Needham, Waddington and Needham, and of others, has shown that one action of the dorso-ventral gradient field of the amphibian egg is to concentrate a chemical substance, the evocator, at the ventral end of the gradient (the organizer). Gastrular ectoderm cells lying above the organizer, even when this is artificially shifted from its proper place, respond to the liberation of the evocator by forming medullary plate. This is a one-way action, comparable to the response of a dog to the smell of a rabbit. In evocation, the relation between the causal and responding agents is an external one, and does not involve the relation which the two agents have to each other as members of the nexus of agents which make up the organism as a whole. This latter relation expresses itself as the individuation fields of Waddington (1933).

In the graded distribution of passive substances such as the evocator, or pigments, or of the character of physiological processes as revealed by the graded susceptibility to poisons in the axial gradients of Child, a morphogenetic field acts as a unit; for these gradations bear no apparent relation to the boundaries of the cells or other parts which together form the region which is the carrier of the field. But the field region is also a nexus of mutually related sub-agents, the cells or other parts just referred to. In its relation to these sub-agents, the field as a whole seems to perform two functions. It provides graded stimuli for them to react to, in this respect functioning as an individuation field. It also influences their reactivity to stimuli emanating from within or without the field—their competence (Waddington) to respond to morphogenetic stimuli.

A similar situation prevails in the case of the Central Agent of the adult animal. Whether it will respond to a specific sensum depends upon its "field"—that is to say, on the nexus of processes forming the brain field at the moment; moreover, the specificity of the response, if it occurs, depends not only on the nature of the sensum, but also on the character of the field at the moment. This is exemplified, for example, in the different responses to the sight of his master by a dog when he has nothing to fear from him, and when he has committed some act which he fears will bring punishment; or of an animal to a sexual object according to whether it is under the influence of sex hormones or not.

The conception of a morphogenetic field as a stimulus field (Reizfeld) has been developed at great length by Gurwitsch (1922, 1930), who also institutes a detailed comparison between the relation of morphogenetic fields to embryonic cells on the one hand, and the relation of brain fields to neurones on the other.

The agents concerned in morphogenesis are, like all living agents, characterised by a specific "action disposition," which the whole of the foregoing discussion implies is a mental disposition. These action dispositions are the morphogenetic potencies of the experimental embryologist.

In an earlier part of this discussion, it was maintained that the distinction between instinctive behavior and reflex action disappears when the agents concerned are properly identified. We may therefore use the word instinct to cover all action dispositions which owe their specificity to factors other than relevant prior experience on the part of the agent concerned. Instinctive behavior is thus contrasted with learnt behavior, though all learnt behavior is a modification of instinctive behavior. There seem to be no grounds for assuming that any learning takes place in morphogenesis. The whole process of morphogenesis is to be regarded as the unfolding of a complex instinctive behavior, involving the distinction we have already drawn between hormic and biological goals, and between immediate and terminal hormic goals. The total instinctive action is not performed by one agent, but is divided up among a progressively increasing number of agents with increasing specialization of action disposition.

It is noteworthy that a comparable situation occurs in the social Hymenoptera and termites. The development from the same germ plasm of different castes with different instincts, is strictly comparable to the development from the same egg of the agents concerned in morphogenesis. In both cases, the agents coöperate to achieve a biological goal which we may confidently assume is not at any time the hormic goal of any one of them. The biological goals are the building up and maintenance of the community in the one case, and of the organism in the other.

Thus we are not drawing a speculative picture when we conceive of a living agent, such as an egg, or germ cell, subdividing into a number of relatively independent agents with specialised instincts which coöperate to produce a biological goal, for this indubitably occurs in nature. The problem is, not whether it is inherently probable or improbable, but whether this occurs in morphogenesis as it does elsewhere.

There is one apparent difference between the responses of the agents in morphogenesis, and those of the "organism as a whole" (the Central Agent). The responses of the former characteristically result in irreversible changes of structure, which has the twofold result of making them incapable of responding again to the same stimulus, and of influencing their competence to respond to stimuli to which they were indifferent before. But irreversible changes may also occur as the result of adult behavior. The act of *learning* changes the competence of the Central Agent to respond to certain stimuli—a change that may last the life time of the organism.

When a rat learns to avoid a lighted alley which is provided with an electric shocking device, the essential change that takes place in the rat is a change in the way it perceives the light. The association of the light and the pain sensa has altered the Central Agent, or its brain field, so that now the light is perceived differently from its mode of perception before the association was formed. Before the experience the Central Agent was indifferent to the light, after the experience it became competent to respond to it.

This seems comparable to the change which takes place in the cells of the amphibian embryo, which in the blastula stage are indifferent to the evocator, but in the gastrula have become competent to respond to it—with the qualification that although the result of the changes from the one condition to the other are comparable, the factors which have brought about that change exhibit one

extraordinary difference—for relevant experience is causally concerned in the one case (learning) but not in the other; this however we have already discussed.

An act of learning is therefore a morphogenetic act, and results in the formation of an action or mental disposition which, as we saw earlier, may be indistinguishable from a disposition produced by the ordinary processes of morphogenesis.

8. PHYSICAL AND TELEOLOGICAL CAUSATION

The principal theoretical difficulty in accepting teleological causation is its apparent incompatability with physical causation. If every event is caused by a preceding event, where can we intercalate teleology into the causal series? In this section we shall try to find assumptions as to the nature of events and their mutual causal relations which will find room both for physical and teleological causation. The discussion will follow closely the principles of Whitehead's system (1929), as I understand it, so far as this is relevant to the special matter under consideration. But although what follows owes to Whitehead everything of value that it may have, I cannot be sure that I have interpreted him correctly in every point, and the extreme brevity of the exposition does not allow full justice to be done to him. Moreover, I have made my own applications of his ideas to the special purposes of this discussion.

The word *event*, as used in this discussion, means a *nexus* of elementary processes so inter-related as to form a whole when considered in relation to its causal effects on other events, including in these the elementary processes themselves which constitute the nexus. A nexus is a pattern, spatial, temporal or both, which is constituted not merely by the spatial or temporal relation of the parts, but by their mutual causal relations. Thus a molecule is a nexus of atoms, and an atom is a nexus of electrons and protons. In every case the elements of the nexus are considered in their dynamic, not in their substantial, character—as *processes* in Whitehead's terminology. Thus an atomic event is that nexus of electronic and protonic processes which endure for the smallest lapse of time necessary to constitute the system in its causal character as an atom. What we call *an atom* is the temporal nexus constituted by a succession of atomic events. Whitehead calls such a nexus, which maintains its character through a series of events, an *enduring object*. The limiting type of event is a single elementary process. Whitehead calls these *actual entities*, or *actual occasions*. As for the purposes of this discussion it is not generally necessary to distinguish between elementary events, and events which consist of a nexus of elementary events acting as a causal unit on other events, I will use the word "event" without always attempting to make this distinction. What I have previously called agents are enduring objects in the above sense.

All elementary physical events last at the most for a very small fraction of a second, but living organisms are characterized by events of longer duration. In man, for instance, the Central Agent is an enduring object, the events constituting which may last for the time interval known to psychologists as the specious present—up to say, $\frac{1}{10}$ to $\frac{1}{2}$ second in duration, or even longer. A single event in the Central Agent is therefore an event with a time duration of this order. It is necessary to bear in mind the conception of an enduring object, such as a molecule or the mind of a man, as a temporal

nexus of events with a certain continuity of character transmitted from one event to another.

Let us now consider physical and teleological causation generally, and specially in relation to the Central Agent.

As Whitehead points out, there are two ways in which we are related both to past and future. We may think about them, in the sense that we can re-construct in imagination the battle of Waterloo, or can speculate on the form our political system will take a hundred years hence. But the past and future are not related to us in this way only. We can feel the causal efficacy of our own past and future. This must first be considered in relation to our immediate past and immediate future—that is to say, the event, or nexus of events, immediately preceding and succeeding the event of the Central Agent occupying the specious present.

If we are making a sustained effort—say, following out a train of thought—we are aware that the character of our present thought (its *subjective form* in Whitehead's terminology) is directly determined by the preceding thought. We are also aware that our present thought is not a terminus, but is directed towards continuing the previous line of thought into the future. Our present thought (more accurately, our present thinking) is an event, growing out of the previous event and leading to the next one.

It is necessary to use other terms than memory and purpose to describe this causal relation of the present act to its immediate past and future, and we can employ Whitehead's terms *prehension* and *subjective aim*. An event of the Central Agent prehends its immediate past, and has subjective aim towards its immediate future.

Let us consider prehension first. To understand this, it is necessary to remember that the Central Agent is not an isolated temporal nexus of events, but forms a part of a larger spatio-temporal nexus of processes which constitutes the brain field, or psycho-physical field of Gestalt psychology. The Central Agent is one temporal series of such processes, possessing a specially intimate coherence. But it is also in intimate relation with the surrounding processes in the brain, and through them, with all the processes in the body. Let us call the totality of processes contiguous to the Central Agent at any moment, its field (though here there is possibly a departure from Whitehead's use of this term).

Prehension is the initial phase of the reaction of an event to its field of contiguous events. Every act of the Central Agent starts therefore by prehending its field of events, an important one of which is the immediately preceding event in the Central Agent itself. But the event has a certain duration; that is to say, certain internal processes have to take place in it before it is in a position to give rise to the next event, and subjective aim concerns this internal process.

It is in this conception of what happens within an event itself that Whitehead departs from ordinary views of the nature of physical causation, which concerns the effect of one event upon another. In any causal series, each event initiates the next; this is physical causation. But each event occupies a certain duration, even if it is only the period of a light wave. What takes place within this duration is outside the physical causal series. Its physical causal nature is that of a process occupying a characteristic temporal and spatial extent. Ultimately, any causal series must be analyzable into a series of events, each the effect of a preceding event, and the cause of a succeeding event, with an interval between the effect and the cause

which is the duration of the event itself. It is in this interval, that is to say, within the event itself, that teleological causation is to be looked for.

According to Whitehead, every event (even such an elementary physical event as an electronic vibration) arises as the effect of a previous event, or nexus of events (its cause), and has subjective aim towards its own completion (*satisfaction*) which will make it a physical cause of the next event. This position can be supported partly by a consideration of our own elementary mental acts, and partly from the physical conception that a chain of physical causation, in its last analysis, is not continuous, but spatio-temporally quantised into finite units. Subjective aim fills the causal gap between the initiation of one event by a previous one, and its own initiation of the next event. As the term implies, it is teleological causation; this is not only because it is directed towards the completion of the present event, but because this completion, or satisfaction, means acting as physical cause of other events. So subjective aim is aim at acting as the cause of other events, (or, following Whitehead's system more closely, aim at determining the character of other events).

But it would seem unnecessary to invoke subjective aim if this was all that was involved. It would be enough to define an event as a process arising as an effect, and becoming itself a cause after a certain spatio-temporal interval, or in other words, that a "causal unit" is not instantaneous, but occupies a small region of space-time (Bertrand Russell). And there seems no evidence that anything more is required for a description of physical processes, or of any process where there is no adaptation to ends. But where adaptation to ends takes place, something must happen within the event itself which will determine that the next event of which it will be a physical cause will be an event appropriate to that end. And this teleological factor is subjective aim. Whitehead, indeed, believes that subjective aim acts in this way even in physical events, but we do not need to follow him here, nor discuss his reasons for this conclusion.

Let us return now to the consideration of purpose in the usual sense of this word. Long range purpose, like memory, depends upon a certain persistence of character of the nexus of processes forming the field of a series of events or acts of the Central Agent. Such purposes have the character of reference to the future; but this is no more nor less mysterious than the fact that memories have reference to the past. And mysterious or not, both are facts. But for science, the problem of purpose is not its reference to the future, but the causal efficacy of this reference, and this is to be found in subjective aim, which concerns the self-completion of each single elementary event which will make it a cause of other events. In conscious behavior the total field of processes which is prehended by the present act of the Central Agent includes memories, sensations and so forth, which together form the action disposition of the organism at the moment, and sets the character of the subjective aim. Subjective aim concerns only a single event of the Central Agent, and long range purposive action is due to a persistent character of the field which is prehended by a series of events constituting a stretch of the enduring object which is the Central Agent. In the higher organisms, this persistent character may be a mental image of the goal. But a consciously conceived purpose, or end in view, is no more necessarily implied in teleological causation than conscious memory is implied in

every case where present action is influenced by past experience.

We have now to examine how teleological causation, or subjective aim, operates. As a preliminary, we may note that mere urge or drive, as such, over a series of events, or within a single event in the form of subjective aim, would not imply teleological causation. At least, it would be useless to invoke teleological causation merely for this. Science is only concerned with entities or functions that cause differences between phenomena, and unless subjective aim means the potentiality of making a difference in subsequent events, it is meaningless. At the very least, teleology, if it is to be causal, must be concerned with existing possibilities of success or failure to achieve an end. We must approach this problem also from consideration of our own purposeful behavior.

Suppose a man is suddenly confronted with a dangerous situation; that is to say, a visual sensum is prehended by the Central Agent in an event which has the subjective form of fear, with the corresponding subjective aim at escape. No teleology is concerned in determining the subjective form and the character of the subjective aim. These are the result of the configuration of the field of the Central Agent. But teleology is concerned with the mode of realization of the subjective aim. For the particular mode of escape—running away, standing still, various kinds of defensive action—depends upon the existing circumstances of the situation. There are various possibilities of action, of which one is to be selected as the one which appears the most suitable to the situation. This selection does not necessarily involve conscious deliberation. Of the various modes of escape, one is perceived as the most suitable.

But possibilities refer to the future, and selection has to operate in the present. What is the present happening that is selected? We will call this thing which is selected, *conceptual action* (partly equivalent to Whitehead's "conceptual feeling of an eternal object"), noting that, as in Whitehead's terminology, conceptual does not imply conscious, though it does imply mental. The forms of conceptual action available to an organism depend upon its bodily organization. For the events forming the field of the Central Agent include those which are the "feelings" of its various bodily parts or sub-agents, caused and maintained by processes in these parts transmitted to the Central Agent. And since conceptual action is to issue in overt action, that is to say, in changes in these bodily parts, the possible forms of conceptual action are dependent on these feelings. The fact that teleological causation has such great scope in living organisms is due to the fact that they are nexus of sub-agents, and therefore rich in forms of conceptual action.

The initial phase of an event in the Central Agent is therefore a prehension of its field of contiguous processes. This prehension gives rise to conceptual action—in fact, conceptual action is the form in which the field is prehended. But various forms of the general conceptual action of escape (in our example) are relevant to the situation, and the most appropriate of these has to be selected and the others rejected. Conceptual actions are possibilities of overt action—but only possibilities, because until one has been accepted by, or incorporated into, the event under consideration, the event does not become the physical cause of other events (in this case, the excitation of motor neurones). "Incorporation" means much the same as Whitehead's *concrescence*.

It must be admitted that the word

"event" seems to have a less precise meaning when used in this sense than when used for an elementary physical event such as a light wave, the spatio-temporal extent of which can be precisely defined. The feature which the two events have in common is that each must be considered as a unit in relation to its causal effect on certain other events. There is clearly a sense in which an event such as we have described in the Central Agent can be analyzed into parts; but considered from the point of view of its causal action in exciting motor neurones, the whole process is a unit. For the selection of the most appropriate form of conceptual action must be considered in relation *both* to the origin and termination of the event. Subjective aim sets the course of the event from its origin as a prehension of a field of events containing a particular visual sensum, to its completion as the physical cause exciting the motor neurones. It is for this reason that teleological causation is located within events, and physical causation at, so to speak, the junction of events. Whitehead, as already mentioned, finds it necessary to ascribe prehension, subjective aim, and satisfaction even to elementary physical events. But without going into this, the comparison with elementary physical events at least seems to show that even in that sphere we have to deal with physical causal units, or processes, which are not instantaneous but occupy a finite region of space-time. Thus no principle of physical causation is violated by locating teleological action between the effect of one link and the cause of the next link in the causal chain.

Perception is a conscious prehension, where the objects prehended are sense data, or sensa, provided by the sensory receptors. Conceptual action is simply the conative (rather than the cognitive) aspect of how the object is prehended or perceived. "Perception in its primary form is consciousness of the causal efficacy of the external world." (Whitehead, 1929, p. 169). I have already discussed this in connection with imagined action, which, when applied to a single event, is conceptual action. But it tends to confuse the issue to use the special term perception only for the prehension of sensa in the usual meaning of that word. For the processes giving rise to bodily pains and fatigues—in fact, any happenings in the "field" of the Central Agent—are perceived in this sense. This means that our own immediate past (the previous specious present) is also perceived. Whitehead (1933) calls this non-sensuous perception.

It is important to remember that teleology is not concerned in the *origination* of conceptual action. A man perceives an orange as something to be eaten, or a rabbit perceives a dog as something to be avoided, because it is their nature to do so—natures which have been determined by their whole past histories, ontogenetic and phylogenetic. If I am trying to solve an intellectual problem, ideas, more or less appropriate, arise in my mind. But these ideas—which are forms of conceptual action in which the conative aspect is at a minimum—arise spontaneously, so far as my will is concerned, according to the psychological principles of association. Teleology is involved in the admission or rejection of these ideas according to whether or not they are consistent with the subjective aim of the event in the Central Agent which has prehended them.

In a train of thought, the adaptation of means to end is carried out by selection of conceptual action which does not lead to overt action. That is to say, the completion of each event of the Central

Agent determines the character of the next event, but does not excite any motor neurones. In conscious thinking, the nexus of successive events of the Central Agent is perceived (in Whitehead's extended meaning of the term) as consciously imagined action, in which, however, the conative aspect may be reduced to a minimum. Typically, thinking is concerned with universals, because adaptive behavior is a fitting of a generalized type of behavior to a particular situation. To quote from Wolters ("On conceptual thinking," 1934):

> The problem of universals (confronts) us in animal behaviour as certainly as in human. A dog recognises any dog as a dog and any cat as a cat, as is clear from the appropriate direction of activities towards members of the two species. . . . The universal is found in the behaviour of the creature. . . . There is a readiness, or preparation, for activity, and it is general in the sense that it is available for a number of situations not completely identical. . . . Where we find this incompletely specialised readiness it is perhaps permissible to call the resulting behaviour "conceptual" in the case of animals as much as of human beings. The name is less important than the recognition that there is a mode of reaction common to men and animals which must be accepted as general in character, but which does not involve the recall to consciousness of any content.

We have discussed teleological causation mainly in relation to the events constituting the Central Agent; but the argument must be applied, in its degree, to all the living agents which constitute the nexus which is the organism as a whole, whether during embryonic development or as the adult: to all agents, that is to say, capable of adaptive action. The essential point is that teleological causation operates within the event itself, by selection of conceptual action. This is a phase of the incomplete event, between its origin as an effect of previous events and its own causal determination of the character of other events.

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THE ORIGIN OF THE FAUNA OF THE GREATER ANTILLES, WITH DISCUSSION OF DISPERSAL OF ANIMALS OVER WATER AND THROUGH THE AIR

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INTRODUCTION AND ACKNOWLEDGMENTS

THE purpose of this paper is to find how the fauna of the Greater Antilles, the four big islands of the West Indies, has been derived from the mainland, whether across the water or over land connections. This problem, like any other in biogeography, involves the recent history of the earth's surface. It is of interest to geologists as well as to biologists, and has been discussed many times, most recently by Schuchert (1935), who concludes that the fauna of the islands has come from Central America over land connections. However, a good deal still remains to be said. Some of what I shall say is elementary, but is necessary for a proper approach to the subject.

It must be remembered throughout that, except where otherwise stated, this paper deals exclusively with the Greater Antilles. The Lesser Antilles have had a different geological origin and have received their fauna largely from different sources, and to lump them in discussion with the Greater Antilles would introduce many complications. The origin of the fauna of the Greater Antilles is a sufficiently complex subject in itself, so much so that, in attempting to simplify it and to keep the length of this paper within bounds, I may occasionally be guilty of "reducing to baldness an argument that is entitled to hairsplitting."

During the writing of this paper I have become indebted especially to Dr. Thomas Barbour, who, although he does not agree entirely with my conclusions, has given me much information and repeated encouragement. Under Dr. Barbour's directorship, the Museum of Comparative Zoölogy is a perfect headquarters for work on the Antilles. I wish it were possible (but it is not) to acknowledge in detail the facts and ideas which have come my way from members of the staff during conversations on the back steps of the museum. Most of the meteorology in this paper has come from or been checked by Prof. C. F. Brooks of Harvard. Information received from specialists is acknowledged at various points in the text below.

THEORY AND MATHEMATICS OF DISPERSAL OF ORGANISMS ACROSS WATER GAPS

Biogeographers customarily refer to dispersal of terrestrial animals across water as "accidental dispersal" or "random dispersal." These terms, however, are bad ones. In this paper the noncommittal term "over-water dispersal" will be used in their place.

The first objection to the term "accidental dispersal", as applied to dispersal across water, is that many factors besides accident are involved. It is no accident that some organisms, because of their nature and behavior, cross water more often than others; and it is no accident that some islands, because of their nature and position, the direction of winds and currents, and the nature of neighboring land, receive more organisms than other islands do. There is, of course, no doubt

that the dispersal of individual land organisms over water is largely accidental (so far as that goes, accidents must occur also in dispersal over land), but in the course of time statistical or mathematical probability comes into play and determines what sorts of organisms cross water most often and what islands they most often reach. Even if there is only one chance in a million that any given individual of a species will cross a given water gap, out of many million individuals some may be sure to cross; and, other things being equal, they will be more likely to cross narrow gaps than wide ones and more likely to populate large and fertile islands than small or barren ones. The dispersal of species and genera may, therefore, be regulated by many factors even when dispersal of individuals is accidental. All this is self-evident.

The mathematical chances of dispersal across water gaps of varying width have not received much attention, and what little has been said about them has usually been wrong. For example, Schuchert (1935, p. 85) quotes and accepts a statement from Matthew to the effect that chances of dispersal of organisms across a barrier vary inversely as the square of the distance. Now the inverse square law, which applies to dispersal phenomena such as the radiation of light, really has nothing whatever to do with dispersal of organisms. Organic dispersal takes place in a plane along the earth's surface, and even if it were otherwise similar to dispersal of light, the simple inverse (as originally suggested by Matthew, 1915, p. 208), not the inverse square, of the distance would be involved. However, this pales to nothing beside a second objection. The inverse square law as applied to light allows for no loss during dispersal, but accidental dispersal of organisms implies the loss of a large proportion of individuals in transit. This is a process not just of a different order but of a different kind.

Take, as an example, two similar islands, one 100 and one 200 miles from a continent. If the chance is 1/1,000 (or 1 in 1,000) that an individual organism which for any reason starts out to sea in the right direction will live and reach the nearer island, the chance that an individual will live and reach the farther island is not $\frac{1}{2}$ as great (as would be true if chances varied with the inverse of the square of the distance) nor $\frac{1}{2}$ (as if chances were in simple inverse to distance) but only 1/1,000 as great, or 1 in 1,000,000, for if only 1/1,000 individuals survive the first 100 miles, only 1/1,000 of the surviving 1/1,000 will be expected to survive the second 100 miles on the basis of chance alone. In general, if X represents the chance of an individual successfully crossing a gap of width m, the chance of an individual crossing a gap of similar nature but of width n should be $(X)^{\frac{n}{m}}$.

This formula is, of course, only an approximation. It represents the chances as determined by the death rate during dispersal, and would hold only if dispersal were in one direction, along a series of parallel lines. If dispersal were in all directions from a single point, chances would vary not only according to the formula $(X)^{\frac{n}{m}}$, but also inversely as the distance, and the formula would be $\frac{(X)^{\frac{n}{m}}}{\frac{n}{m}}$. Since, however, dispersal must usually be from numerous points along a coast line of appreciable length, the chance of gap n being crossed will usually be between the values given by these two formulae. If X' is taken to represent the chance of an individual crossing n, the full formula becomes

$$(X)^{\frac{n}{m}} \gtrless X' \gtrless \frac{(X)^{\frac{n}{m}}}{\frac{n}{m}}.$$

For the purposes of the present paper, however, $(X)^{\frac{n}{m}}$ is a sufficiently close approximation.

In the case of the two similar islands described above, $X = 1/1{,}000$; $m = 100$; and $n = 200$; and the chance of an individual crossing n and reaching the outer island is not over $(1/1{,}000)^2$, or 1 in 1,000,000. If there were a third similar island 300 miles at sea, the chance of an individual reaching it would be not over $(1/1{,}000)^3$, or 1 in 1,000,000,000. This is less than 1/100,000 as good a chance as would be figured from Matthew's inverse square law! If, on the other hand, there were 1/1,000 chance that an individual would reach the 300 mile island successfully, the chance of one reaching the 100 mile island would be not less than $(1/1{,}000)^{\frac{1}{2}}$, or 1/10.

I shall not apply this formula very closely to specific existing islands, for I think it is a mistake to apply mathematics too closely to biological phenomena of which we know as little as we do of dispersal of organisms. In nature, mathematical chances must be modified by many biological factors, as well as by differences in age and availability of different islands, direction of storms and currents, etc. The formula $(X)^{\frac{n}{m}}$ is not exact; it is merely a convenient mathematical approximation. All I wish to conclude from it is that the relative widths of water gaps are more important in dispersal of organisms than is usually realized, and that organisms which are dispersed across water from a continent to a series of islands are correspondingly likely to reach the nearest islands first and very unlikely to skip near and populate far islands directly. In other words, the chances are that organisms dispersed across water to islands will be distributed in an orderly fashion, the order depending to a great extent on the widths of different water gaps.

Biologists writing on the fauna of the West Indian islands have repeatedly stated or implied not only that dispersal across water is accidental, but that the distribution pattern which results is accidental and irregular, with related species scattered at random. These persons believe that organisms dispersed across water can be recognized by their erratic distribution as contrasted with the orderly distribution of organisms which have been dispersed over land connections. (See especially Barbour, 1910, p. 280, and other papers; and Schuchert, 1935, pp. 105–106, agreeing with Barbour's statements.)

Actually, however, the formula $(X)^{\frac{n}{m}}$ shows that dispersal over water should result in a fairly orderly distribution of life, with related forms occurring in orderly series on adjacent islands, much as they would be expected to do if dispersal were over land connections. The formula $(X)^{\frac{n}{m}}$ shows, too, that the greater are the odds against dispersal of individuals and the more rare and accidental are the events which bring dispersal about, the more orderly should be the resultant distribution of species. If this is a paradox, it is a reasonable one. A strong-flying bird, for example, does have a better chance of getting from Cuba to Puerto Rico without becoming established on Hispaniola than a mammal has of being rafted around Hispaniola without landing there, and the bird is more likely to become irregularly distributed. It seems, then, that erratic distribution among islands, when it reflects organisms' characteristics at all, indicates *ease of dispersal of individuals;* not that dispersal has been by "accident"; and not that it has been by water rather than by land. Of course erratic distribution among islands must often be the result not of organisms' characteristics at all, but of differences in the age or suitability of the islands or in other inorganic factors.

All this leads to the conclusion that, even when the dispersal of individual organisms across water to islands is largely accidental, the fauna and flora which accumulates upon the islands is largely not accidental, either in composition or in distribution. There is no obvious reason why such a fauna and flora should be less orderly than a fauna and flora dispersed over land, for there are plenty of irregularities of distribution to be found on continents. Mere order in the distribution of life on a series of islands is not, therefore, necessarily evidence of former land connections. The real differences between a biota dispersed over water gaps and one dispersed over land connections are to be looked for, first, in the nature of the constituent organisms—whether or not they are mostly of sorts easily dispersed over water and found on oceanic islands—and, second, in the *details* (not the degree) of their orderliness—whether their distribution reflects the widths of different water gaps and the direction of winds and currents, or the direction of past land connections.

FACTORS CONCERNED IN OVER-WATER IMMIGRATION OF ANIMALS INTO THE GREATER ANTILLES

Widths of water gaps, present and past

To turn to the Greater Antilles, and to the question of the origin of their fauna, it is necessary to examine not so much the present geography of the region as its geological history, in order to get an idea of the conditions under which animals have reached the islands.

At present Yucatan, about 125 miles from Cuba, is the nearest continental land to the Greater Antilles. The mainland of Florida, however, is less than 140 miles from Cuba. The distance from South America to the Greater Antilles is relatively great, but is spanned by a chain of islands, the Lesser Antilles, with the widest water gap only about 90 miles. It is impossible to say what the relative chances of immigration of animals from different directions are under these conditions.

In the past, however, the distribution of land has been very different. The peninsula of Florida has existed only since the Miocene, and its southern tip only since the Pleistocene. Some of the Lesser Antilles are rather recent, too, dating probably from the Pliocene or Pleistocene (cf. Schuchert, 1935, p. 749), and the islands have probably never been connected together north of Barbados and St. Vincent (Schuchert, pp. 730–731), although there are differences of opinion about this. Yucatan, too, is recent; the outer part of the peninsula became land only in the Pleistocene. There are, however, banks off Honduras and Nicaragua which, although they are now under the ocean, are thought by geologists to have been elevated for long periods in the past. These banks may have formed a continuous land bridge from Central America to Jamaica and Cuba, or the bridge may have been incomplete, interrupted by relatively narrow water gaps.

In the past, then, chances of immigration of animals into the Greater Antilles from Central America have probably been much better than now; chances of immigration from other directions, poorer than now, although there is some disagreement about the Lesser Antilles. Whether the Honduras-Nicaraguan route was continuous dry land, or whether there was always a water gap, is a question which geology cannot answer and which Schuchert (pp. 109–110) has referred to biology.

It is true that Schuchert (1935, p. 109) says, "If the latter [geologists and stratigraphers] know anything at all, it is that the present Greater Antilles

are not oceanic or volcanic islands but consist of continental rocks and are now fragments of a once greater Central America", and it is true that Schuchert's palaeographic maps (following p. 767) show wide and persistent connections between the Greater Antilles and Central America; but, on the other hand, Schuchert also states perfectly definitely (pp. 109–110) that there is really no geological proof that the islands have ever been connected with the mainland at all. Schuchert's maps give some idea of the probable conformation of the Caribbean region with the origin of the islands' fauna. The matter cannot be dismissed so easily, however. The trades may never reverse themselves except at very high altitudes, but the records show that hurricanes sometimes do move from the mainland to the islands (Fig. 1). For example, in 1905 a hurricane originated with its center just off the east coast of Nicaragua,

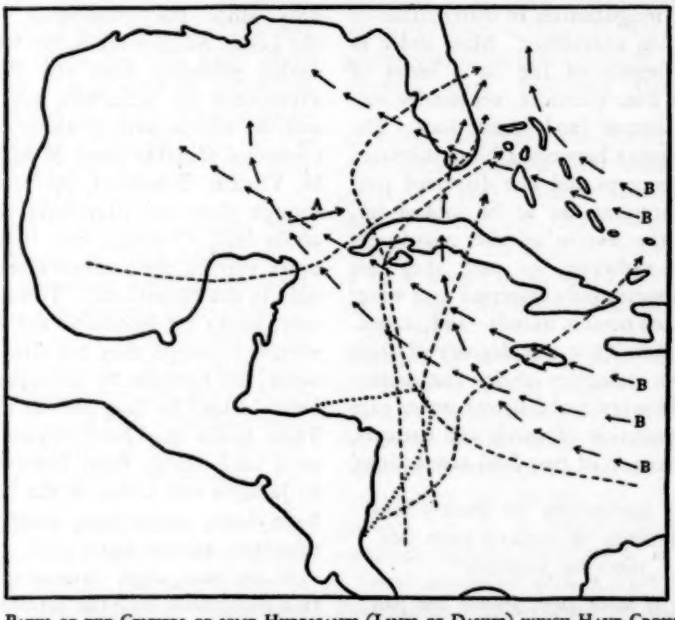

FIG. 1. PATHS OF THE CENTERS OF SOME HURRICANES (LINES OF DASHES) WHICH HAVE CROSSED FROM OVER OR NEAR CENTRAL AMERICA OR THE HONDURAS-NICARAGUAN BANKS TO THE GREATER ANTILLES (FROM SOURCES CITED IN TEXT)

Hurricanes like these are exceptional but not really rare. (Most West Indian hurricanes come in from the southeast; several typical paths are shown by series of arrows, BB.) Arrow at A shows approximate position and direction of progression of a hurricane center when wind of maximum velocity blows from Yucatan to Cuba; hurricanes followed approximately this path in 1895 and 1906.

in the past, but, in view of what has just been said, they must be used cautiously.

Direction, nature, and lifting power of winds

The winds of the Greater Antillean region, both trades and hurricanes, usually trend from the islands to the mainland, and for this reason it has usually been supposed that winds have had nothing to do so close that the land must have felt the effects of the storm, and then crossed directly over Jamaica and over the eastern end of Cuba, and very close to Haiti. In 1906, a hurricane originated in the Gulf of Mexico and crossed the Yucatan Peninsula from west to east, and the storm center moved northeast across the Yucatan Channel and just north of

western Cuba, which must have received the most severe part of the storm. Besides the few hurricanes which have traveled from the present boundaries of Central America to the Greater Antilles, others have crossed from above the submerged Honduras-Nicaraguan banks to the islands. If these banks were elevated, as geologists suppose they were, the number of storms crossing from the mainland to the islands would be increased. In any case, several hurricanes are known to have crossed from Central America to the Greater Antilles during the last 50 years, and many thousands have probably done so during the history of the islands. (Fassig, 1913; U. S. Weather Bureau map of hurricane paths from 1887 to 1929; *Cyclonic Storms*, U. S. Hydrographic Office, 1936; Tannehill, 1934; etc. may be consulted for further information and for maps of hurricane paths.)

It is necessary to know something about the structure of hurricanes to understand their possible importance in the dispersal of organisms. Hurricane winds are cyclonic or nearly circular, and, in the northern hemisphere, rotate about the storm center in a counter-clockwise direction (Fig. 2). The speed of progression of the storm center is rather slow, averaging only about 12 miles an hour in the tropics, but the cyclonic wind is often over 100 miles an hour, with gusts of 200 miles an hour or more. The ground velocity of the wind is greatest to the right of the path of the hurricane center, where the speed of progression of the storm is added to the rate of rotation; least at the left, where the ground velocity is rotational speed minus the speed of progression. The highest winds are usually not far from the storm center, but in very large hurricanes "destructive" winds may occur as far as 200 or 250 miles from the center, which means that they may blow across a gap of 100 miles or more with little curvature.

As the land now lies, almost any hurricane which passes through or near the Yucatan Channel may cause a wind to blow for a time from Yucatan to Cuba, no matter in what direction the storm center is moving. The wind will reach its greatest velocity from Yucatan to Cuba, however, during the passage of a storm toward the east along a path a little

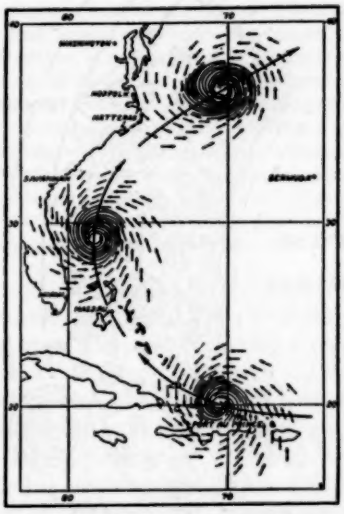

FIG. 2. DIAGRAM OF TRACK AND WIND SYSTEM OF A WEST INDIAN HURRICANE (FROM TANNEHILL, 1934)

north of the narrowest water gap (Fig. 1, A). Under the most favorable conditions the wind probably crosses the gap in not much over an hour, certainly in less than two hours. Storms followed the optimum path in 1895 and 1906 (the latter a different storm from the one mentioned above in the same year).

The lifting power of hurricane winds is, of course, as important as direction and velocity in dispersal of organisms. The center of a big hurricane is usually calm

for a diameter of from 7 to 20 miles. The inner cyclonic wind, nearest the calm center, angles upward, and the rate of ascent probably increases when the cloud zone is reached, for release of heat during condensation of rain forces an updraft of warm air which, it is believed, supplies the motive power of the whole hurricane. The central updraft of a hurricane is very

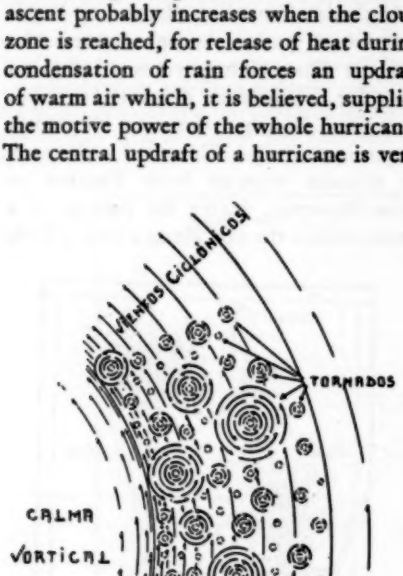

FIG. 3. DIAGRAM TO SHOW PROBABLE TORNADO-LIKE NATURE OF GUSTS CARRIED BY A HURRICANE WIND (FROM VÁZQUEZ, 1936)

strong, and carries many thousand feet high. The main hurricane wind, however, except near the center, probably of itself has little lifting power, but the main wind carries with it gusts which seem to be of the nature of secondary whirls or tornadoes, and the secondary whirls sometimes have great lifting power. Violent tornadoes are known occasionally to have been carried by even the outer winds of hurricanes. For instance, in 1919 a tornado carried by a moderate hurricane wind moving at about 60 miles an hour, about 125 miles from the center of the main storm, cut a path 14 miles long and 100 to 600 feet wide southwest of Miami, Florida (Gray, 1919). Similar secondary whirls or tornadoes, varying in size and violence, are probably common near the centers of hurricanes. Vázquez (1936) gives a diagram (reproduced here, Fig. 3) of a hurricane wind literally packed with secondary tornadoes. It is interesting that one of the bits of evidence given by Vázquez to prove the tornado-like nature of hurricane gusts is the fall of live shrimp during hurricane squalls, which shows that the secondary whirls are sometimes strong enough to form waterspouts. One can get some idea of the sort of whirls or eddies which must occur in wind flowing at 100 miles an hour over irregular country, with a ground pressure of about a ton to the square foot, by watching the eddies in a stream of water flowing comparatively slowly over an irregular bed.

Not very much is known, really, of the details of structure of hurricanes even at ground level, and less at high altitudes, so that it is impossible to say how high secondary whirls carry debris or how long they hold it, or whether debris carried up by one whirl may be thrown into another or into the main updraft of the storm. But, from what is known, it is reasonable to suppose that hurricanes sometimes pick up fairly heavy objects and carry them rapidly and for long distances.

Men instinctively underestimate the lifting power of air. Many zoölogists to-day find it as difficult to believe that winds and rising air currents can lift and carry moderately heavy organisms as

persons once found it to believe that heavier-than-air machines could fly. There are several reasons why this should be so, why men should fail to appreciate the power of rising air currents and their effect on small organisms.

First, men live on the ground and are rarely exposed to rising air. They come to think of winds as chiefly horizontal forces, although meteorologists know that vertical air currents are really very common higher above the ground.

Second, men are large animals, and often do not realize the effect of air currents on smaller forms. Small animals are, of course, very much more at the mercy of winds than large ones are. The difference is due not directly to the difference in weight, but to the fact that small animals have a much higher ratio of surface to weight. The volume (and weight) of animals is a product of *three* dimensions and varies approximately as the cube of one dimension; the surface area of the body is a product of two dimensions, and varies approximately as a square. (Cf. the formula for the volume of a sphere, $\frac{4}{3}\pi d^3$, and that for the surface of a sphere, πd^2.) It is not necessary to demonstrate the mathematics in detail here, but it can be found that an animal 1 inch long has 65 or 70 times the surface for its weight as a similarly shaped animal the size of an average man. The ratio can be figured also from weight, and it can be found that an animal weighing 1 ounce (an adult house mouse weighs about 1 ounce) has about 14 times the surface for its weight as a similar animal the weight of an average man. These figures would have to be modified to allow for differences in shape as well as size, but it is not necessary to go into this complication here. Small animals certainly have many times more surface for their weight than large animals do, and air currents, which act upon the surface, have a proportionately greater effect upon small animals.

Finally, the pressure exerted upon a given object by wind varies not directly with the wind velocity but about as the *square* of the velocity. A wind of 100 miles an hour exerts about 16 times the pressure upon a given object as a wind of 25 miles an hour.

The formula for wind pressure upon flat surfaces, and its application to cylindrical objects, is briefly discussed by Humphreys (1929, p. 641). To calculate exactly the pressure of winds of different velocities upon objects of irregular shape is extremely difficult. It is hard enough to calculate the pressure upon simple spheres, as may be seen from Bilham's (1937) calculation of the rate of ascending air currents which support large hailstones. (It is interesting to note that, in theory, the largest spherical hailstone which can be supported by rising air currents other than whirlwinds is about 5 inches in diameter, although this maximum is hardly ever attained in nature.) However, the square of the velocity gives at least a rough idea of the relative power of a wind.

A wind of 100 miles an hour, then, has something like 224 (14×16) times the effect upon an animal weighing 1 ounce, and something like 1,040 (65×16) times the effect upon an animal 1 inch long, as a wind of 25 miles an hour has upon a man.

A man's experience, therefore, upon which his instinctive judgment is based, is that of a *large* animal, exposed for the most part to *moderate* winds, blowing *horizontally* near ground level. The effect of more severe winds upon small animals, even small fishes and frogs and mammals, may be hundreds of times greater in proportion to weight. And once an animal is carried above the ground by a whirlwind, it enters a zone where rising air currents are common. It is true that the figures cited above are only rough approximations, but they make it clear that a man cannot from his own experience estimate the effect of severe winds on small animals. Whether or not the latter

are carried by winds can be determined only by observation or experiment, and there are, of course, many actual observations which show that winds do carry small animals.

McAtee (1917) has given an excellent brief review of the transportation of various objects by wind and of the falling of such animals as earthworms, frogs, and small fishes with rain. Gudger (1911, 1929) has summarized about 70 more or less well authenticated falls of fishes in different parts of the world. There is no doubt that these phenomena occur and that the animals which fall have been carried up by whirlwinds or in waterspouts. Sometimes the animals come down alive, sometimes dead. Dr. Carl L. Hubbs has written me (letter of June 10, 1937) that, to test the possibility of dispersal of living fishes in waterspouts, he once dropped a number of small live fishes off the roof of a building, catching some in a pail of water and allowing others to strike on various wet and dry substrata. Most of the fishes lived.

The possibility of hurricane dispersal of some specific Greater Antillean animals will be discussed in more detail below. From what has already been said, however, about the direction, nature, and lifting power of hurricane winds, and about the ability of winds to carry small animals, it is clear that hurricanes must be taken into account in studying the origin of the islands' fauna.

Among the hurricanes which cross every few years from Central America to the Greater Antilles are some very violent ones. Such severe storms rarely cross from any other continental region directly to the islands. Only the relatively inefficient backdraft at the left of northward bound hurricanes blows from Florida to Cuba. The hurricanes which occasionally cross from the coast of Venezuela to the Greater Antilles usually follow a long, diagonal course. Storms rarely travel from South America up the Lesser Antillean chain, and the few which do so are usually not of hurricane violence. If hurricanes carry organisms at all, therefore, they probably have carried chiefly Central American forms to the Greater Antilles.

Besides hurricanes, small, local, cyclonic storms, like the tornadoes of the United States, sometimes occur in the Antilles. In Cuba, for instance, (according to data secured for me by Dr. S. C. Bruner) there are probably one or two such storms a year on the average. They are most common in the western part of the island, and usually occur in the warmer months, from June to October. Sometimes they form waterspouts. The Cubans call them, appropriately, *rabos de nubes* (cloud tails). Such local storms probably play some part in the dispersal of Antillean organisms, although they progress much less rapidly than tornadoes carried by hurricane winds, and are less likely to carry across wide water gaps. Even smaller and less violent whirlwinds may be of some importance in dispersal of island organisms. Perkins (1913, p. xlviii.) describes how, in the Hawaiian Islands, small whirlwinds sometimes carry columns of dust up more than 2,000 feet, and may carry small organisms into high air currents.

Ocean currents

Chances of dispersal of perishable organisms by ocean currents in the Antilles are probably not so good as by winds, although resistant seeds and animals which can stand long submergence in the sea must constantly be carried about. (See Guppy, 1917, for data on seed dispersal by ocean currents.) The course of the Gulf Stream seems not to favor the rapid transportation of organisms from the mainland to the islands. Local surface currents set up by hurricane winds, which sometimes blow very hard for a day or two in one place and direction, at one side of a nearly stationary storm, may

carry some organisms in floating debris, especially since hurricane rains are apt to flood rivers and wash out much drift material, but it must be remembered that the sea is very rough at such times.

Past differences in climate

It is likely that, in the past, meteorological conditions in the Antillean region have been very different from what they are now. It is probably idle to speculate much about this except for one thing. Before the Pleistocene, the climate of the whole world was much more uniform than it is now. The trade winds, which now sweep regularly from the Greater Antilles to the continent, owe their strength to the difference in temperature now existing between the poles and the equator. When the temperature differential was smaller, the trades were probably weaker and possibly more erratic, and the direction of progression of hurricanes, which depends partly on the direction of the prevailing winds, may have been more erratic too, so that chances of storm and drift dispersal in various directions may have been much better than now.

However this may be, the known facts suggest that the position of the islands, with the narrowest water gap on the Central American side in the past, and the direction and nature of storms have favored immigration of organisms into the Greater Antilles chiefly from Central America, even if there has never been a complete land connection.

Possible means of dispersal of Antillean animals across water, and answers to arguments against an over-water origin of the Antillean fauna

The most common argument against an over-water origin of the Greater Antillean fauna is that there are animals on the islands which *cannot* have crossed water barriers in any way. However, from what I have seen during collecting trips to Cuba, Jamaica, and Haiti, I think that this is incorrect, and that there is really no Antillean animal the ancestor of which cannot reasonably be supposed to have crossed water.

There are no animals on the Antilles too large to have been rafted across water gaps. It is probably true, as Barbour (1916) has said in criticism of Matthew (1915), that chances of raft dispersal are not good in the West Indies to-day, but in the past, before Pleistocene differentiation of polar and equatorial climates, the prevailing winds were probably lighter and the sea smoother, and chances of dispersal by natural rafts may have been better. Dispersal of delicate animals which cannot stand salt water may have been through the air. I have tried to show above that chances of aerial dispersal by hurricanes are very much greater than is usually realized. Animals which cannot be carried about free in the air might be carried in plant debris, and there is much debris in Central America and the Antilles capable of carrying animals. There are, for example, various palms which have huge leaves with sheath-like leaf bases.

According to Dr. L. H. Bailey (in letter of May 4, 1937) the West Indian royal palms (Roystoneas) have the most extensive leaf sheaths of any palms. The continental palms most like them are a few species of Euterpe in Guatemala, Nicaragua, and Costa Rica. Dr. Bailey points out that there are other plant structures, especially the floral spathes of some palms, which might carry organisms. Ordinary rolled dead leaves, too, especially large ones like those of the common Cecropia tree, might carry small organisms. However, I have chosen the palm leaves for special discussion because they are large and may carry relatively large animals; because palms of the right sort are native in both Central America and the Antilles; and because I know something of the fauna of fallen palm leaves.

The leaf bases of these palms are very large, six or eight feet long and two or three feet wide. When a leaf dies and falls to the ground, the base usually rolls up. The dead leaf is then about as near as nature can get to a kite four or five yards long with a large, tubular basket at one end. Sometimes the leaf base rolls into a smooth cylinder from which the contents might easily be shaken, but sometimes it rolls irregularly or becomes choked with trash. Such a leaf is fairly heavy, especially when water soaked, but it is nevertheless far lighter for its area than many objects which are known to have been carried by wind. McAtee (1917, pp. 217–218) gives some examples of the lifting power of whirlwinds. They have been known to carry even solid wood and metal for considerable distances.

The rolled boots of fallen palm leaves actually are favorite hiding places of many Antillean animals. The larger animals occupy the cavity at the center of the roll; smaller ones often wedge themselves between layers of rolled tissue. I have often collected in palm boots not only a variety of insects and ground mollusks and other invertebrates, but snakes and lizards and frogs. I have seen earthworms in palm boots over which damp leaves had drifted. *Peripatus* probably occurs in palm boots, too, although I have never actually found one there. Dr. A. M. Chickering writes (letter of Aug. 4, 1937) that he has collected *Peripatus* in fallen palm leaves on Barro Colorado Island, Panamá Canal Zone—not, indeed, in palm boots, but against the midribs of the leaves, between leaflets which had folded together. A single rolled palm boot may house a large number and variety of small animals such as these.

It seems to me that any ground-living animal which does not weigh more than a few pounds *might* be carried in the rolled boot of a dead palm leaf by a hurricane. Even small mammals might be carried, especially the young, in nests. The tough, rolled palm boot, jammed with nest material, would give a good deal of protection both in the air and in landing, and a nest might contain more than one young, which would increase the chance of a species becoming established in new territory. It is, however, doubtful if viviparous fishes could be carried in palm leaves, although the latter often fall boot-first into ponds and become choked with mud and water weeds.

Neither Dr. Barbour nor Dr. Hubbs thinks it likely that freshwater fishes have reached the Antilles through the air. Dr. Hubbs (letter of June 10, 1937) suggests that some or all of the few sorts of freshwater fishes which have reached Cuba could live for some time in sea water, and that they may possibly have crossed through the sea, possibly at a time when heavy rains or a large river had somewhat sweetened the sea's surface.

Of course the best way we have of knowing whether particular sorts of animals really do cross ocean barriers is by whether or not they occur on known oceanic islands, and this test shows that birds and bats and insects and land snails do sometimes cross very wide water gaps. See Gulick (1932) for some of the biological peculiarities of oceanic islands, but remember that the islands with which Gulick deals are much more remote and much smaller than the Greater Antilles. Unfortunately no evidence of this sort is available for many kinds of Antillean animals, or rather the evidence is doubtful because of doubt as to the history of certain islands.

For example, it is supposed by most geologists and many biogeographers (cf. Schuchert, 1935, pp. 108, 729–730, 731, 732) that the Lesser Antilles, north of Barbados and St. Vincent, are true oceanic islands which have never been connected by land. If so,

their fauna, which comprises most of the classes of animals found on the Greater Antilles, including some terrestrial mammals and *Peripatus*, but probably not true fresh-water fishes, has reached the islands across water. Unfortunately, however, although there is no doubt that most of the Lesser Antilles are volcanic islands and not remnants of a continuous ridge, the islands stand upon a shelf which is nowhere deeper than 1,000 fathoms and for the most part less than 500, and the shelf may possibly at some time have been elevated so as to permit migration of animals.

There are, then, animals on the Greater Antilles of sorts which are not known on definitely oceanic islands. It cannot be proved that such animals are able to cross ocean barriers. Nevertheless, as I have tried to show above, they may have done so. Objections to over-water origin of the Greater Antillean fauna merely on the ground that some of the animals could not in any way have crossed water do not seem to be valid.

It may be asked why, if animals have reached the Greater Antilles over water in the past, they do not do so now. A few small animals, including even some reptiles and a frog, may have reached Cuba rather recently, for they belong to mainland genera not found on any of the islands except Cuba. However, among such groups as the terrestrial mammals it seems that no species has reached the Greater Antilles for a very long time, except for species introduced by man. This, though, does not bar out the possibility that mammals came over water in the past, when weather conditions may have been more favorable to rafting and when water gaps were probably narrower.

Suppose that the ancestors of the Greater Antillean mammals reached the islands across a water gap of 80 miles, about the distance between the Rosalind and Pedro Banks if the banks were elevated 100 fathoms. Then what would be the chances, relatively speaking, that mammals would cross the present 125 mile gap between Yucatan and Cuba? The formula to use is $(X)^{\frac{n}{m}}$, and $\frac{n}{m} = \text{about } \frac{3}{2}$. If there were 1/100 chance that an individual mammal would cross the Rosalind-Pedro gap successfully, the same mammal would have less than 1/10 as good a chance to cross the present Yucatan Channel; if the chance of the first crossing were 1/10,000, that of the second would be less than 1/100 as good; etc. And if dispersal has been by hurricanes, there is an additional factor to be taken into account. Hurricane winds (Fig. 2) are nearly circular and are much more likely to blow directly across narrow gaps than wide ones. The width of a gap must be even more important in dispersal by hurricanes than the formula $(X)^{\frac{n}{m}}$ shows.

All this suggests that animals which may have been reasonably likely, in the course of a long period of time, to cross a gap of 80 miles from Central America to the Greater Antilles may have almost no chance at all to cross the present gap.

Another objection to over-water dispersal which has been raised repeatedly (Allen, 1911; Barbour, 1914, p. 236; Bates, 1935, p. 89) is based on the existence of local endemic subspecies and species (of bats, reptiles and amphibians, and butterflies) confined to different Antillean islands or to the islands as opposed to the mainland. It is argued that, if over-water dispersal occurred, it would cause intermixing of populations and would prevent the divergence of local forms, so that the very existence of the latter is evidence that there has been no dispersal across the water gaps. This reasoning, however, does not stand up against either mathematics or logic.

As an example, let us take a hypo-

thetical island inhabited by a species *X*, of which there are a million individuals reproduced in one generation a year, and let us suppose that on another island there is a related interfertile species *Y*. If one individual of *Y* reaches the first island each year and interbreeds with *X*, what will be the result? A simple calculation shows that, if only one part per million of *Y* germ plasm is introduced into *X* each year, and if there is no selection, it will require a considerable part of a million years for much effect to be produced. Selection of more than one *X* at the expense of one *Y* per million individuals, or any other equivalent evolutionary process, should more than overcome the effect of intermixture. Even if 100 individuals of *Y* interbreed with *X* each year, selection of more than one *X* over one *Y* per ten thousand individuals, or any other equivalent force tending toward divergence of *X*, should more than overcome the effect of intermixture. An evolutionary force equivalent to selection of one individual in ten thousand is surely not an unreasonably strong force. Yet arrival of even one individual a year on a suitable but unpopulated island is probably a much higher frequency than would be necessary to ensure establishment of a species. Local endemics might, therefore, originate even if dispersal were constantly occurring at a rate much higher than necessary to populate islands.

If this mathematical demonstration is not enough, two analogies ought to be convincing. First, endemics have frequently originated on small portions of continents, although migration must at some time have taken place over the land. How, then, can the presence of endemics on islands be taken to prove that migration was not at some time over water? Second, the Hawaiian Islands are probably true oceanic islands and the Hawaiian fauna has probably been derived from great distances across water, yet some of the animals, for instance many birds and beetles, have evolved local forms on different islands of the Hawaiian group which are separated by very narrow water gaps. If the Hawaiian animals really arrived across water (which, it must be admitted, a few persons doubt), the Antillean animals may also have done so, so far as the evidence of endemic forms goes. There is no doubt, of course, that water gaps are important barriers, but there is no evidence that they are completely impassable for any Antillean animals.

Nothing which has been said thus far has been intended to prove that animals really have reached the Greater Antilles over water, merely that they may have done so; that conditions have probably been much more favorable for over-water immigration than has usually been realized, and that the objections to over-water dispersal are not very serious. Whether the islands really have been populated over water or across land connections is, I think, a question which can be answered only by a study of the fauna itself, not by arguments about chances of dispersal of animals across water gaps. A study of certain characteristics of the Greater Antillean fauna will be attempted in the following pages.

CHARACTERISTICS OF GREATER ANTILLEAN FAUNA

It is pretty generally agreed that the fauna of the Greater Antilles has three chief characteristics. First, it is derived principally (but not wholly) from Central America, except that many of the birds have North American affinities (Bond, 1934), and that it is impossible to say with certainty from what direction the terrestrial mammals arrived. Second, the fauna is fairly homogeneous and very

orderly; the islands differ considerably among themselves, but they are faunistically much more like each other than like the mainland, and related animals are usually distributed in orderly series. Third, the fauna is in many ways strikingly depauperate; many groups of continental animals which are common even in Central America are absent on the islands. The multiplication of species and sometimes even of genera in some groups of animals on the islands, through radiative evolution and setting off of localized forms, should perhaps be mentioned as another characteristic of the fauna.

Fauna derived chiefly from Central America

Under this heading it is necessary only to repeat what has been said earlier in this paper, that the conformation of land in the past, with the narrowest water gap on the Central American side, and the direction of hurricanes have favored immigration of animals into the Greater Antilles chiefly from Central America even if there have never been complete land connections. The relationships of the fauna with Central America, therefore, do not necessarily indicate a Central American land bridge, nor can geologists say for sure whether or not such a bridge has ever existed.

Homogeneity and orderliness of fauna

The homogeneity and orderliness of the Greater Antillean fauna have frequently been invoked as evidences of past land connections, at least among the different islands. The homogeneity is only moderate, but the orderliness is striking. For example, there are five genera of frogs and toads in the Greater Antilles, but only one genus (*Eleutherodactylus*) occurs on all four islands. This is a poor score for homogeneity. Every one of the five genera, however, is distributed in an orderly way, upon a series of adjacent islands or, if a single island, one which is adjacent to the mainland. The genus *Hyla* is found in Cuba, Hispaniola, and Jamaica, but not in Puerto Rico; *Bufo*, in Cuba, Hispaniola, and Puerto Rico, but not in Jamaica; *Leptodactylus*, in Hispaniola, Puerto Rico (and the Lesser Antilles), but not in Cuba or Jamaica; *Phyllobates*, only in Cuba. I have chosen frogs and toads as an example because they are well known and not too readily dispersed, and because they include no endemic Antillean genera. The evidence in some other groups is incomplete or complicated by doubt of the exact relationships of endemic forms. On the whole, however, in spite of a few exceptions, the distribution of related forms in orderly series is a striking characteristic of the Greater Antillean fauna.

The significance of this orderliness is, I think, very different from what has usually been supposed. The mere fact that order is present does not necessarily mean that dispersal has been across land connections. This has been pointed out in the first part of this paper, and some details of the orderliness of the Greater Antillean fauna will be discussed further below. What the orderliness shows most clearly is that the present Greater Antillean fauna has accumulated through series of immigrations at a time when the conformation of the land was not so very different from what it is now. Whether the immigrations were over land or over water is a question which may be postponed for the moment. The present fauna is a product of immigration, and there has been no wide-spread extinction of animals since the fauna originated. It is just about inconceivable that, if the present fauna were derived from a larger fauna of continental type by extinction of

many forms, the remnants would be distributed in such an orderly way, along what appear to be original migration routes. I think it is safe to conclude, then, that the Greater Antillean fauna is an *accumulation* of immigrants, not the *residue* of a larger fauna.

The terrestrial mammals seem at first to be evidence against this, for there is no doubt that many Antillean mammals have become extinct. A good many of them have become extinct only through human agency, however, and if all known forms, extinct as well as living, are considered, they are found to exhibit much the same moderate homogeneity and greater orderliness as the rest of the fauna. Ground sloths are known from Cuba, Hispaniola, and Puerto Rico, but not from Jamaica. Insectivora, too, are known from Cuba, Hispaniola, and Puerto Rico, but not from Jamaica; *Solenodon* is known only from Cuba and Hispaniola; *Nesophontes*, from all three islands. Rodents are more numerous and occur upon all of the Greater Antilles, but most of them are hystricomorphs of a few related groups. It is true that future discoveries of fossils may change this picture, but the facts as now known suggest that the mammals as well as the other Greater Antillean animals were, until the recent extinction of so many forms, distributed in an orderly way, and that the mammal fauna, too, was an accumulation of immigrants, not the residue of a continental fauna.

There seems, in fact, to be no significant exception to the orderliness of the Greater Antillean fauna. It should be noted especially that, although different animals have probably reached the islands at different times, the fauna as a whole does *not* divide into a group of older residual forms and a group of more recent immigrants. The frogs and toads are among the most ancient of existing land vertebrates—older in their origin than placental mammals—but no Greater Antillean animals are distributed in a more orderly way. If the frogs and toads are descended from a few immigrant genera, as their distribution seems to show, it is hard to believe that any Antillean vertebrates can be relicts of a continental fauna.

Irregular depauperateness of fauna

Island faunae probably always tend to be depauperate even on recent continental islands, for the size of an island probably limits to some extent the number of animals which can exist on it. The Greater Antilles, however, seem to lack many more forms of life than their size warrants. The depauperateness of the fauna is uneven, much more striking in some groups than in others. Several examples are given below. In analyzing them an attempt will be made to find whether or not the groups which are poorest in the Greater Antilles are those least likely to cross water barriers, and whether or not the groups which are best represented are those characteristic of oceanic islands.

Warm-blooded vertebrates. (I am indebted to Dr. G. M. Allen for reading and correcting this part of the paper.) Terrestrial mammals are very poorly represented on the Greater Antilles. The entire native fauna, including extinct forms, consists of two unrelated genera of primitive insectivores, three or four groups of primitive rodents, and ground sloths. One gets the impression from Matthew (1919, pp. 171–172) that this entire fauna might have been descended from six or seven ancestral immigrant types. It is impossible to say definitely from what direction the different forms reached the islands, for none has close relatives living anywhere on the main-

land. However, the insectivores probably came originally from North America; the sloths and rodents, from South America. The best guess probably is that they all reached the islands somehow by way of Central America.

Among the groups of animals which are absent (except for recently introduced forms) upon the Greater Antilles are perissodactyls, artiodactyls, proboscidians, carnivores, modern insectivores, and non-hystricomorph rodents, except that one of the latter (a species of *Oryzomys*) once occurred upon Jamaica and may have been endemic. Even more significant than the absence of these groups, most of which are of northern origin, is the absence of certain groups which have gone through long evolution in South America. For example, marsupials, anteaters, armadillos, the several peculiar groups of South American ungulates, and South American monkeys are completely unrepresented by either living or fossil forms on the islands, and even the hystricomorph rodents are represented by only a fraction of the groups which have existed in South America. (See Lydekker, 1896, Chapter 3, for an account of the history of mammals in South America.) There never was a time when free communication with either North or South America would have been expected to bring such a limited fauna to the Greater Antilles. A very large amount of fossil material—fragments of certainly thousands of individual mammals—has been recovered on the islands in cave and spring deposits of Pleistocene age. It is practically certain that there were no mammals larger than ground sloths and large rodents on the islands when these fossil deposits accumulated, and it is unlikely that many even of the smaller forms escaped preservation. Nothing is known, however, of the mammal fauna of the islands before the Pleistocene. Matthew (1918, 1919) gives further data and compares the Antillean mammal fauna with that of other islands.

According to Chapman and J. A. Allen (1893, pp. 231–234) the following terrestrial mammals occur on Trinidad, a recent continental island smaller than the smallest of the Greater Antilles, and only about 1/20 the size of Cuba: monkeys (2); carnivora (5 species, in 3 families); a squirrel, several mice, and other non-hystricomorphs as well as hystricomorph rodents; a deer; peccaries (2); the two-toed sloth; anteaters (3); an armadillo; and opossums (3). The presence of these animals on Trinidad shows that a relatively small island can support a much more diverse mammal fauna than is found on the Greater Antilles.

Bats, in contrast to the ground-living mammals, are relatively numerous and diverse on the Greater Antilles. According to Anthony's list (1926, pp. 208–211) the islands have 17 genera of bats in common with the mainland. There are also 10 endemic genera, most of which seem to be derived from mainland rather than from island forms. The minimum number of immigrant stocks necessary to have produced the Greater Antillean bat fauna is probably about 27, or about four times the minimum necessary to have produced the fauna of ground-living mammals. This is remarkable when it is remembered that bats are less numerous than ground-living mammals in continental faunae. It is true that it is impossible to be sure how many immigrations have really occurred in any group of Greater Antillean mammals, but it cannot be doubted that bats are disproportionately numerous and diverse and that they have reached the islands relatively often. Birds, too, are relatively numerous and diverse, so ob-

viously so that there is no need to cite figures.

G. M. Allen (1911) once tried to show that the bats as well as the ground-living mammals reached the Antilles over land connections. However, he did not make out a very good case, partly because he took the existence of local endemic forms as proof of complete isolation, partly because of the fact that bats are known to have reached various oceanic islands across water. (I learn from Dr. Allen that he now thinks that most of the Antillean bats may have been distributed over water.) Bond (1934) supposes that even most of the resident birds originally reached the Greater Antilles by land bridge via Central America. If this is so, it is a remarkable coincidence that birds, which are the only animals to migrate extensively to the Antilles from the north, show exceptionally strong North American relationships even among resident species. Did most warm-blooded vertebrates of all kinds reach the Greater Antilles by land from Central America? Or does the scarcity of flightless and the relative abundance and diversity of flying forms indicate immigration across water? For my part, I think that over-water immigration is indicated, and that the exceptional northern affinities of the resident birds indicate immigration of their ancestors from North America across the water gap. Remember that birds even more than bats have reached and populated oceanic islands.

Amphibians and reptiles. The following table gives the number of genera and species of frogs and toads, lizards, and snakes (turtles will be mentioned later) known from Cuba, Puerto Rico, and (for comparison) Trinidad, the only undoubted continental island of anything like comparable size in the Caribbean region. The figures for Cuba and Puerto Rico are from the latest West Indian checklist (Barbour, 1935); those for Trinidad, from Parker (1933, 1935) and Mole (1924).

	CUBA (AREA ABOUT 40,000 SQ. MI.)		PUERTO RICO (AREA ABOUT 3,400 SQ. MI.)		TRINIDAD (AREA ABOUT 2,000 SQ. MI.)	
	Genera	*Species*	*Genera*	*Species*	*Genera*	*Species*
Frogs and toads.....	4	22	3	13	12	23
Lizards.....	15	46	9	22	15	19
Snakes.....	8	14	4	6	28	38

Cuba is the largest and Puerto Rico the smallest of the Greater Antilles. The figures for Hispaniola (about 30,000 square miles) follow almost exactly those for Cuba, although some of the genera and most of the species are different; the figures for Jamaica (about 4,500 square miles) follow closely those for Puerto Rico.

This table shows that the amphibians and reptiles of the Greater Antilles are poor out of all proportion to the islands' size, especially when genera are considered. Species are relatively numerous, for endemic species have multiplied, especially on the larger islands. As a matter of fact even a few of the genera (of lizards and snakes) may be endemics derived from other Antillean forms. The number of original stocks necessary to have produced the Greater Antillean fauna may be even smaller than the figures show.

As compared with the fauna of Trinidad, that of the Greater Antilles is not only poor but is characterized by a predominance of lizards as compared with frogs and especially with snakes. On Cuba and Puerto Rico there are more genera of lizards than of snakes and amphibia together, while on Trinidad the *snake* genera outnumber the other two groups together. One or two of the Greater Antillean lizard genera may have been

introduced by man and one or two others may be endemic and derived from other island genera, which somewhat reduces the number of original generic stocks, but one or two of the snake genera may be endemic too. There is no doubt that lizards are disproportionately numerous and diverse on the Greater Antilles. Wallace (1876, p. 28; 1911, pp. 279–280) long ago pointed out that, for some unknown reason, lizards seem to be better able than frogs or snakes to cross ocean barriers, and more often occur on oceanic islands. The Greater Antilles, then, resemble oceanic islands not only in the limited nature of their fauna of amphibians and reptiles, but in the predominance of lizards.

Butterflies. Bates (1935, p. 83) states that there are 289 species of butterflies known from Trinidad, only 156 from Cuba. On p. 88 he says further," . . . . exceptions seem to me of comparatively little weight when compared with the striking general occurrence of wide ranging, strong flying butterfly groups in the West Indies, and the absence of butterflies belonging to groups characterized by weak flying, non-migrating and local habit." This certainly suggests immigration of butterflies across a water gap wide enough to bar out weak flying, non-migratory forms, but, as Bates says, we do not know enough about the general habits of butterflies to draw final conclusions.

Fresh-water fauna. Briefly, some of the characteristics of the fresh-water fauna of the Greater Antilles are these: There is only one genus of fresh-water turtles, with several very closely related species. Some of the Antillean frogs have freed themselves from dependence upon open water by suppressing the tadpole stage in favor of direct development. This, however, is characteristic of frogs of some continental as well as some oceanic islands (Barbour, 1926, pp. 83–85). Strictly fresh-water fishes are not known at all from Puerto Rico, and those of Cuba and Hispaniola are descended from a very few stocks as compared with those of Trinidad.

My information about West Indian fishes is from letters from Dr. George S. Myers and Dr. Carl L. Hubbs, and from conversation with Dr. Thomas Barbour. Dr. Hubbs' letter is particularly quotable. He says, under date of April 6, 1937, "On the problem of the freshwater fish fauna of Puerto Rico, you should refer to the paper by Hildebrand in *Copeia*, 1935 (2). He shows that the fauna as listed comprises 2 freshwater fishes almost surely erroneously so recorded; several species introduced by man; the eel (1) (his species) which breeds in the sea; *Poecilia vivipara* (2) which also occurs through the Lesser Antilles to South America but is absent in Cuba, and which occasionally occurs in brackish or even salt water, though belonging to a freshwater family; *Agonostomus* (3) which occurs generally on isolated islands and presumably in some way passes through the sea; several gobies (4 to 9) that are more or less transitional between marine and fluviatile types. Of the gobies *Sicydium* ranks with *Agonostomus* in being an upland, swift-water inhabitant of isolated lands.

"The Lesser Antilles have virtually the same fish fauna as Puerto Rico. The description of a *Cichlasoma* from Barbados is now considered to have been based on a mislabeled Cuban specimen.

"Trinidad, in contrast, has a rather large freshwater fish fauna, of South American affinity, as long ago pointed out by Gill (*Ann. Lyc. Nat. Hist. N. Y.*, 6:363–430).

"The Greater Antilles [excepting Puerto Rico] have a moderate freshwater fish fauna, in number of species, but one that is very depauperate in number of types. That is, in agreement with Dunn, we (Howell Rivero and I) have found and will show in a later paper that there is evidence for only a few migrations of freshwater fishes from the mainland (clearly from Central America) onto the Antilles. But several of the types after attaining the islands obviously underwent radiative adaptation, thus increasing the fauna in species and genera."

Among insects which live or breed in fresh water, stone flies (Plecoptera) seem to be completely absent in the Antilles.

There are some caddice flies (Trichoptera) and some May flies (Plectoptera) on most or all of the Greater Antilles, although the fauna of these orders is not rich. All of these groups are weak in flight. More strongly flying aquatic insects, such as dragon flies and water beetles, are usually better represented. Among mollusks, according to Mr. W. J. Clench, the fresh-water groups are very poor compared with those of the land, although the difference is due partly to the great multiplication of land forms.

On the whole, then, although information on the subject is far from complete, the fresh-water fauna of the Greater Antilles seems to be relatively poor. This may be due partly to the fact that, on some of the islands, there is little fresh water available except in small, rapid brooks. However, this is probably not the only factor involved. The adaptation of certain brackish-water fishes to fresh water, the radial evolution of some other fishes, and the relative abundance of certain strong-flying and readily dispersable aquatic insects suggest that the fresh-water habitat is adequate and that various aquatic animals are absent or poorly represented because of barriers to dispersal. Wallace (1911, pp. 304–305) and others have pointed out that paucity or absence of fresh-water life is characteristic of remote oceanic islands, and to a limited extent the Greater Antilles show this characteristic, whatever the reason may be.

Radiative evolution in significant groups

There is some question as to just how much radiative evolution has occurred in different groups of animals on the Greater Antilles. The terrestrial mammals may have evolved on the islands from only six or seven ancestors. Nothing is known of the history of their immediate mainland ancestors, however, so part of their evolution may possibly have occurred before they reached the islands. Dunn (1932) believes that the amphibians and reptiles of the Greater Antilles have arisen by radiation of species and even genera from relatively few immigrants, "extraordinarily few when one considers the enormous number of Central American genera"; but Dr. Barbour tells me that he believes that Dunn overestimates the amount of radiation which has occurred actually upon the islands. Hubbs and Rivero (letter quoted above) believe that there has been extensive radiation among the fresh-water fishes of the Greater Antilles. And there has certainly been an enormous amount of radiation among the land mollusks. There has, I think, unquestionably been a good deal of radiation in all of these groups upon the Greater Antilles, in spite of details which are in doubt. However, it is hard to be sure just what this signifies. Radiative evolution occurs upon continents as well as upon islands, and amount of radiation must depend upon the age of an island's fauna as well as upon the mode of origin. Nevertheless, conspicuous radiation is probably especially characteristic of the faunae of oceanic islands, as Perkins (1913, pp. lii–lxi) has found it to be of the presumably oceanic Hawaiian fauna. The radiation which has occurred in the Antillean fauna, in conjunction with the other evidence, certainly suggests an over-water origin, but should not alone be given too much weight.

Significance of characteristics of fauna

The preceding discussion covers only a fraction of the Greater Antillean fauna, leaving most of the invertebrates out of

consideration, but the groups which have been discussed include the *best known* animals and those which, because of their large size and the difficulty which some of them have in crossing salt water, are most significant zoögeographicaliy. These significant groups show that the fauna of the Greater Antilles has certain characteristics of the faunae of oceanic islands, at least in a modified form. Not only is the fauna depauperate, but the groups which are poorest seem to be those least likely to cross water barriers; many of the groups which are best represented are characteristic of oceanic islands; and there has been a good deal of radiative evolution in the most significant groups.

At first thought there seem to be three possible explanations of these facts. *First*, if the fauna of the Greater Antilles were the residue of a larger fauna of continental type, the present characteristics of the fauna might have arisen by extinction of animals unfitted for, and survival and multiplication of animals fitted for life on islands. There are, however, several objections to this. The orderly distribution of animals on the Greater Antilles, along what appear to be routes of immigration, makes it practically certain that the fauna is not a residual one. Comparison with continental islands such as Trinidad and the East Indies does not show that a fauna would be so highly modified by mere existence upon islands, although differences in age of different islands makes this evidence questionable. And the animals which are lacking or poorly represented on the Greater Antilles are in some cases of sorts that are well suited to existence in isolated habitats if they can get there. For example, sluggish, flightless insects are uncommon in the Antilles except for those which have lost the power of flight locally on certain mountain ranges (cf. Darlington, 1937), but flightless insects are well fitted for life on islands, on some of which they are very common.

Perkins (1913, pp. xlviii and following) mentions a large number of flightless insects which inhabit the Hawaiian Islands. Jeannel (1925) discusses some cases in which flightless beetles have survived successfully upon islands in the eastern Atlantic Ocean. Flightless birds, too, frequently occur on islands. There is every reason to think that sluggish, flightless animals are well suited to island life.

It appears, then, for various reasons, that the faunal peculiarities of the Greater Antilles are not the result of large-scale elimination of animals unsuited to island life.

Second, it might be supposed that migration over some special sort of land bridge has produced a fauna with the special characteristics observed. The difficulty is to say what sort of land bridge could produce the required result. If the fauna really has come over a land bridge, the latter must have been extensive enough and must have lasted long enough to carry fresh-water fishes and a variety of slowly moving land mollusks. No such bridge could have barred the many sorts of animals which are absent or poorly represented on the Antilles. A low, flat bridge like Florida would be no bar. A great many mammals, for example, have invaded the peninsula of Florida, including (according to the list published by Outram Bangs, 1898) the opossum, shrews, moles, various carnivora, diverse rodents, a deer, and (fossil) proboscidians, various ungulates, and other large forms. The amphibian and reptile fauna of Florida, too, has entirely different characteristics from that of the Greater Antilles. The numbers of genera of frogs and toads, lizards, and snakes in Cuba have been found to be respectively 4, 15, and 8; in Florida the numbers are 7, 8, and 25 (figures from

Stejneger and Barbour, 1933) and there are 13 genera of non-marine turtles in Florida as compared with 1 living genus and 1 extinct one (a giant land tortoise) in Cuba. (One genus of frogs and two of lizards which have been introduced into Florida are omitted from these figures.) There are in Florida also 10 genera of caudate amphibians, while the only Antillean record for the Caudata is now thought to be an error, but most of the Floridan caudates occur in the northern part of the state and have not penetrated the peninsula. A few genera of the other groups are limited to northern Florida, but the peninsular fauna is nevertheless much more diverse than that of the Greater Antilles. Moreover the snakes are dominant in Florida as they are on the continental island of Trinidad, while lizards are dominant on the Greater Antilles. Fresh-water fishes, too, are abundant and diverse in Florida. There is, then, no reason to think that a low, flat land bridge would have produced, by selection of immigrants, a fauna like that of the Greater Antilles. Nor is a mountainous land bridge (suggested by Schuchert, 1935, p. 110) more satisfactory. North and South America are now joined by a mountainous land bridge through Central America, and an enormous variety of animals is known to have crossed the bridge, including a great many mammals from proboscidia and large carnivora down. And how could a mountainous land bridge which could carry even a few fresh-water fishes to the Greater Antilles be supposed to bar anything else?

There remains the *third* possibility, that the Greater Antillean fauna is depauperate and shows some of the characteristics of the faunae of oceanic islands because it has been derived in the same way, across water. This, in my opinion, is the only reasonable explanation of the facts.

Distribution formulae

There is one other source of evidence on the mode of origin of the Greater Antillean fauna. It is the distribution and interrelationships of the animals on different islands. If land bridges have existed in the past, the present distribution of animals ought to reflect them; and if dispersal has been across water gaps, the distribution of animals ought to reflect that, too. It will be interesting, therefore, to try to decide what the relationships of the animals on different islands ought to be in different hypothetical cases, and to compare the results with the relationships which actually exist.

According to Schuchert (1935), if the Greater Antilles have been connected with the continent (and remember that Schuchert says, p. 110, that there is no geological proof that there has ever been a connection), the connection has probably been from Honduras to Jamaica, and has probably extended *by way of Hispaniola* to Cuba, or at least this has probably been the most recent series of connections.

What Schuchert actually says (p. 108) is, "As the writer sees the physical evidence, the Antillean basin broke down from the Gulf of Mexico across the Central American geanticline, first cutting off Cuba from Central America, then sending its waters east to the south of Cuba, next separating Jamaica from Cuba but not from Haiti, and eventually cutting Cuba off from Hispaniola; thus, the Honduras-Jamaica-Hispaniola bridge was the last part of the Antillean geanticline to break down."

If this has really been the geological history of the Antillean region, the relationships of the animals should be expressed by the following formula, the closeness of relationships being inversely proportional to the length of the dashes:

Honduras–Jamaica–Hispaniola—Cuba—— Yucatan; Jamaica——Cuba

If, however, the relationships have been determined by the widths of existing water gaps (Fig. 4), the formula should be something like this:

Honduras —— Jamaica — Hispaniola–Cuba—Yucatan; Jamaica—Cuba

But the relationships actually shown by the most significant groups of animals, i.e. those, like mammals, amphibians and reptiles, and land mollusks, which are fairly well known and not too readily dispersed, are something like this:

Honduras —— Jamaica – Hispaniola – Cuba—Yucatan; Jamaica—Cuba

Certain details of this last formula are doubtful. There is, for instance, doubt as to whether Jamaica is really faunistically as close to Hispaniola as Cuba is. Anthony (1916, p. 198) says, "The mammalia of Jamaica is sufficiently aberrant in many details to warrant the assumption that it was probably split off from the main Antillean continent, if such existed, before the other three islands were broken apart." Possibly, however, the mammalian evidence (mostly fossil) is too incomplete to be trusted. The amphibians and reptiles were at one time considered to show a greater isolation between Jamaica and Haiti than between Cuba and Haiti, but Dr. Barbour tells me that recent discoveries indicate that Jamaica no less than Cuba is closely related to Hispaniola in its reptile fauna. Simpson (1894, pp. 434, 435, and 436) believed that Jamaica was shown by evidence of the land mollusks to be the most isolated of any of the Greater Antilles. This, however, was a long time ago; more complete collecting may show a closer relationship than was supposed between Jamaica and southwestern Haiti.

There is some doubt, too, as to whether the Cuban fauna is really closer than the Jamaican to that of the mainland. In amphibians and reptiles Cuba certainly has more groups in common with the continent (Barbour, 1914, pp. 224–225), but the evidence from other significant groups is not, so far as I have been able to find, very satisfactory.

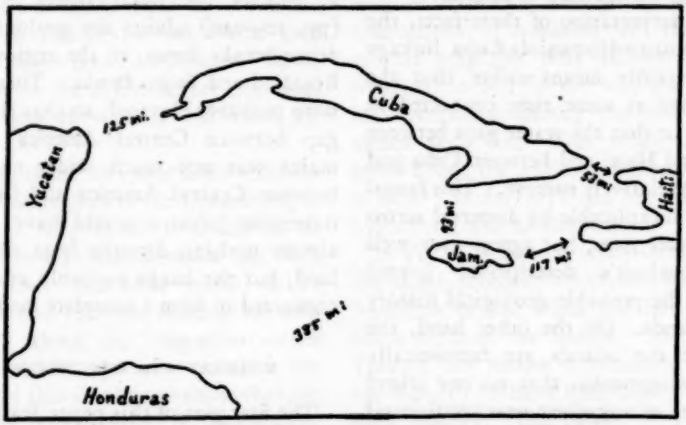

FIG. 4. OUTLINE MAP TO SHOW APPROXIMATE WIDTHS OF WATER GAPS IN WESTERN ANTILLEAN REGION

Arrows show direction of strongest faunal relationships. Note absence of strong relationships between Jamaica and Cuba, although water gap is narrower than that between Jamaica and Haiti.

In spite of doubt about details, there are two main facts expressed in the last formula which are beyond doubt. First, faunal relationships among the islands are definitely from Jamaica to Hispaniola to Cuba (Fig. 4), not directly from Jamaica to Cuba. Barbour (1910, p. 277; 1914, pp. 227–228) has pointed this out for the amphibians and reptiles; Bates (1935, p. 90) shows that the rule holds

remarkably for the butterflies; Mr. Clench tells me that it holds for the land mollusks; from my own experience I think it holds for the beetles; it appears to hold, with rare exceptions, for the whole fauna, except for some readily dispersed forms. The fauna shows an unmistakable Jamaica-Hispaniola-Cuba linkage. Second, the Greater Antilles are faunistically much more closely related to each other than any one of the islands is to the mainland; so far as I know, this is unanimously agreed upon by zoölogists.

As to interpretation of these facts, the definite Jamaica-Hispaniola-Cuba linkage almost certainly means either that the islands were at some time connected in this order or that the water gaps between Jamaica and Haiti and between Cuba and Haiti were relatively narrow. This faunal linkage is inexplicable by dispersal across present water gaps, but agrees very well with Schuchert's description, quoted above, of the probable geological history of the islands. On the other hand, the facts that the islands are faunistically rather homogeneous, that no one island has a fauna of anywhere near continental type, and that Jamaica is if anything faunistically more isolated than Cuba both from the other islands and from the mainland, and cannot possibly have been the center of dispersal of more than a fraction of the Greater Antillean fauna, exactly contradict Schuchert. According to the latter, the geological evidence is that, if there were ever land connections from the continent to the islands, the most recent was from Honduras to Jamaica. In fact this is said (p. 108) to be the only connection since the Triassic which is probable on geological grounds. Yet the distribution of the fauna does not reflect such a connection.

To give two examples more specific than those cited above, there are three orders of mammals (all placental mammals originated later than the Triassic) known from Cuba and Hispaniola, but only one of them, Rodentia, is known ever to have occurred in Jamaica. And of the five genera of frogs and toads in the Greater Antilles only two occur in Jamaica.

It cannot be said that the Antillean fauna is too mixed to show the direction of past connections, for it does plainly show the Jamaica-Hispaniola-Cuba linkage. I think that the conclusion must be that there was no connection with the mainland; that there was always a water gap at exactly the point where Schuchert (pp. 109–110) admits the geological evidence breaks down, in the region of the Rosalind and Pedro Banks. These banks were probably elevated, so that the water gap between Central America and Jamaica was not much wider than that between Central America and Cuba, for otherwise Jamaica would have received almost nothing directly from the mainland, but the banks probably never were connected to form a complete land bridge.

SUMMARY AND CONCLUSIONS

The first part of this paper deals, partly mathematically, with the chances of dispersal of organisms across water gaps and among islands, and concludes that, although the dispersal of individual organisms across water is partly accidental, the distribution of species which results is largely not accidental and not haphazard, but reasonably orderly, the order depending much more than is usually realized upon the relative widths of water gaps.

Next, the possibility of dispersal of animals across water from the mainland of America to the Greater Antilles is considered. It is found that the position of the islands, the geological history of the region, and the direction and nature of winds are such that animals may well

have been transported across water from the mainland to the islands by storms as well as in other ways, and it is found that the chances of such transportation have been best from the direction of Central America, the region from which most of the Greater Antillean fauna has actually been derived. Several common objections to over-water dispersal of Antillean animals are examined, and none is found to be a very serious objection. At this point in the paper it appears merely that animals *might* have reached the Greater Antilles across water gaps as well as across land connections. However, none of the evidence yet examined indicates how they really did reach the islands.

The last part of the paper deals with the actual characteristics of the Greater Antillean fauna. The fauna is found to be only moderately homogeneous but very orderly. The orderliness seems to be due to the fact that the animals are still distributed along the migration routes by which their ancestors reached the islands, and this is taken to show that the fauna is an accumulation of immigrants, not the residue of a continental fauna. As to whether the immigrants arrived over land or over water, there seem to be only two sources of real evidence. One is the nature of the fauna as a whole. The fauna is irregularly depauperate and shows various characteristics of an oceanic fauna, and the only reasonable explanation seems to be that immigration was across water gaps wide enough to bar out, partly or completely, those sorts of animals least able to cross water. The other is the distribution of animals among the islands. The distribution reflects no *specific* connection with the mainland, although it does reflect probable connections among the islands, between Jamaica and Hispaniola and between Cuba and Hispaniola. In particular there is no indication of a relatively recent connection from Honduras to Jamaica, although, according to geology, that is where the most recent connection with the mainland should have been, if there ever was a connection.

Both of the two independent lines of valid evidence indicate, then, that the fauna of the Greater Antilles has been derived from the mainland across water, although dispersal from one island to another may have been partly over land connections. Derivation of the fauna in this way not only accounts for its composition and for the way the animals are distributed among the islands, but agrees very well with what is really *known* about the geological history of the islands. So far as I can see, no other hypothesis will fit the facts.

In Fig. 5 is shown what seems to me the most probable conformation of the Antillean region when the bulk of the fauna reached the islands. Florida and Yucatan are shown submerged, as they are thought to have been during most of their history. The banks off Honduras and Nicaragua are shown not only elevated but extending much farther toward Cuba than they do now. So far as the distribution of animals goes, the approximation of Central America to Cuba might have been accomplished equally well by an elevation of the Campeche Bank off Yucatan, but the extension from Honduras is more likely on geological grounds. I should guess that the narrowest water gap toward Cuba was about 75 or 80 miles; toward Jamaica, slightly wider. I cannot justify this guess to the mile, but at least it seems likely that the gaps were narrower than the present Yucatan Channel, which is about 125 miles wide, while, on the other hand, they were wide enough to impress upon the fauna

of the islands some of the characteristics of an oceanic fauna. There may, of course, have been a series of two or more gaps instead of the single one shown. There may also have been narrow gaps between Jamaica and Hispaniola and between Cuba and Hispaniola, but these gaps, if they existed, were probably much narrower than the gap between Jamaica and Cuba.

And if hurricanes followed the same paths they do now, many of them would pass over the peninsula to the islands.

There seems, then, to be no good evidence for even one land bridge to the Greater Antilles. Yet if a land bridge theory were to be adopted at all, at least three bridges would probably be required, one to Cuba, another *independent* one to Jamaica, and a third from South America

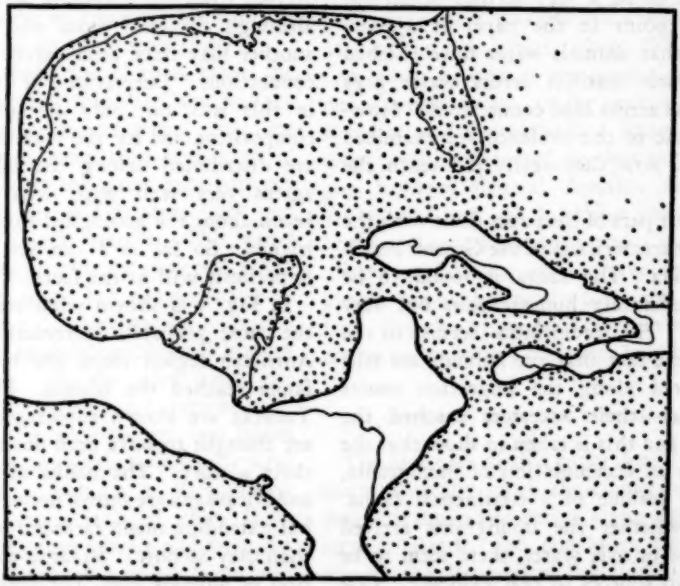

FIG. 5. POSSIBLE CONFORMATION OF LAND (HEAVY LINES) IN WESTERN CARIBBEAN REGION WHEN MOST OF THE GREATER ANTILLEAN FAUNA WAS DERIVED FROM THE MAINLAND

Present land indicated with light lines. The Bahaman region has intentionally been left blank; it, and the Lesser Antilles, are outside the scope of this paper.

If the land did lie as shown in Fig. 5, chances of dispersal of animals across the water were probably very good indeed. The force of the trades would be broken by the union of Cuba, Hispaniola, and Jamaica, forming a gulf of relatively calm water favorable to rafting. The Gulf Stream might eddy or follow a course such as to carry drift from the Honduras-Nicaraguan peninsula to Cuba. up the Lesser Antilles, for animals have certainly reached the islands from all these directions. (Several zoögeographers have claimed a fourth bridge direct from northern Africa to the Antilles, and at least one person has argued for a bridge from *Asia* to the Antilles but not touching either North or South America!) To give up any one of the three main land bridges would apparently be to admit

that certain mammals and reptiles, as well as other animals, have crossed water gaps, and once it is admitted that such forms as these cross water barriers at all, the chief argument for land bridges in the Antilles is gone. Any practiced land bridge enthusiast can draw a set of maps showing how a series of land connections might have produced the relationships shown by the Greater Antillean fauna. The difficulty is to match the connections with geological evidence, and to explain why, if there have been several connections with the mainland, the fauna of the islands shows so many characteristics of an oceanic fauna. It is hard to imagine even one land bridge sufficiently selective to have produced these characteristics.

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SYNCHRONOUS RHYTHMIC FLASHING OF FIREFLIES

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I. INTRODUCTION

ONE of the most interesting and complex types of group behavior in animals is that in which several organisms simultaneously repeat the same activity at regular intervals of time. In ordinary usage such behavior is called "synchronous," but, as pointed out by Craig (1916) and Alexander (1935), it actually involves two distinct factors, synchronism and rhythm.

Neither synchronism nor rhythm, alone, is uncommon. Synchronous behavior, for example, often occurs in a group of organisms as the result of a common response to the same stimulus. This stimulus may either be environmental, as in the synchronous responses observed in saw-fly larvae (Bennett, 1860), caterpillars (Beebe and Beebe, 1910), plant lice (Allard, 1917), fireflies (Allard, 1935), etc., or it may appear to originate in some individual in the group, as in the simultaneous behavior of birds (Annandale, 1900; Craig, 1916; Wheeler, 1917), pronghorned antelope (Wheeler, 1917), fish (Beebe, 1926), and frogs (Cunningham, 1903; Allard, 1917). Rhythm, likewise, is a common phenomenon in animals (Flattely, 1920), the chirping of crickets (Dolbear, 1897; Bessey and Bessey, 1898; Edes, 1899; *et al.*), and the beat of the heart being two examples among many. Lillie (1928) has pointed out several interesting analogies between physiological rhythms and those in inorganic systems.

The following pages deal primarily with types of behavior which involve both synchronism and rhythm. Such behavior has been reported in a wide variety of organisms, e.g., in ants (Peal, 1880, 1881; Forbes, 1881; Gounelle, 1900; *et al.*), Phalangidae (Newman, 1917; Wheeler, 1917), web-worm larvae (McDermott, 1916; Peairs, 1917), Orthoptera (Gould, 1895; Coues, 1895; Bostwick, 1895; Dolbear, 1897; Shull, 1907; Craig, 1916, 1917; E. S. Morse, 1916 a; Allard, 1917, 1918, 1930; Comstock, 1924; Fulton, 1925, 1928 a, 1928 b, 1934; *et al.*), caterpillars (Allard, 1917; Buxton, 1923; Minnich, 1925), bees (Burbidge, 1880; Evans, 1922), marine organisms (Herdman, 1903, 1903 a, 1905) wasps (Beebe, 1921), aphids (Williams, 1922; Tanner, 1930), termites (Conner, 1933), and many others. Since, however, by far the largest number of reports concern synchronous rhythmic flashing of fireflies, the subsequent discussion is confined primarily to these insects. There is presented a rather extensive review of the earlier literature, found in obscure journals and books of travel, and a more superficial review of the more recent literature, found in accessible journals. Among the many friends who have aided in the preparation of this paper, I wish in particular to thank Dr. E. W. Gudger who generously placed at my disposal a large number of references to synchronism.

The earliest account found is in Kaempfer's (1727) description of a trip down the Meinam River from Bangkok (Siam). He says:

> The Glowworms (*Cicindelae*) represent another shew, which settle on some Trees, like a fiery cloud, with this surprising circumstance, that a whole swarm of these Insects, having taken possession of one Tree, and spread themselves over its branches, sometimes hide their Light all at once, and a moment after make it appear again with the utmost regularity and exactness, as if they were in perpetual Systole and Diastole.

Turpin, in a book on Siam (1771) says (translation): "Nothing gives a finer sight at night than to see a tree all covered with fireflies; it seems decked with many bright sparks which go out and rekindle almost at the same moment."

Goldsmith (1811) states: "The trees on the banks of the Meinam are finely illuminated with swarms of fireflies, which emit and conceal their light as uniformly as if it proceeded from a machine of the most exact contrivance." (See also Van Vleck, 1924.)

Bishop Pallegoix (1854) reports (translation):

> The fireflies which are found in Siam are very common and are remarkable for the intensity of their light. One sees these insects fly separately from one tree to another and in all directions, but more often they are assembled by thousands on a great tree on the bank of the river. It is a magnificent spectacle to see spring out at one time from all the branches of this tree as it were, thousands of great electric sparks, because these fireflies emit not a continuous light but one interrupted by the effect of a sort of respiration. It is difficult to explain how this emission of light is simultaneous for several thousands of individuals.

Sir John Bowring (1857) poetically remarks of the Siamese firefly: "They have their favorite trees, round which they sport in countless multitudes, and produce a magnificent and living illumination: their light blazes and is extinguished by a common sympathy. At one moment every leaf and branch appears decorated with diamond-like fire; and soon there is darkness, to be again succeeded by flashes from innumerable lamps which whirl about in rapid agitation."

Cameron (1865) describes observations made in Burma:

> The bushes literally swarm with fireflies, which flash out their intermittent light almost contemporaneously; the effect being that for an instant the exact outline of all the bushes stands prominently forward, as if lit up with electric sparks, and next moment all is jetty dark—darker from the momentary illumination that preceded. These flashes succeed one another every three or four seconds for about ten minutes, when an interval of similar duration takes place; as if to allow the insects to regain their electric or phosphoric vigor.

A very interesting discussion of Cameron's report is found in *The Transactions of the Entomological Society, London*, Ser. III, vol. 2, pp. 94–95, 1865, in which the Rev. H. Clark reports having seen synchronous flashing in the Organ Mountains (Brazil?).

Theobald (1866) gives a full description of a display witnessed along the banks of the Irawadi Delta in Burma; which is quoted here in part:

> The bushes overhanging the water were one mass of fireflies. . . .The light of this great body of insects was given out. . . .in rhythmic flashes, and for a second or two lighted up the bushes in a beautiful manner; heightened, no doubt, by the sudden relapse into darkness which followed each flash. There are the facts of the case (and I may add that it was towards the end of the year) and the only suggestion I would throw out, to account for the unusual method of luminous emanation, is that the close congregation of large numbers of insects, from the small space afforded them by the bushes in question, may have given rise to the synchronous emission of the flash by the force of imitation or *sympathy*.

Collingwood (1868) says, concerning fireflies at Singapore:

> . . . .on fine evenings and in favorable (that is, damp and swampy) localities, they present a very remarkable appearance. Clustered in the foliage of

the trees, instead of keeping up an irregular twinkle, every individual shines simultaneously at regular intervals, as though by a common impulse; so that their light pulsates, as it were, and the tree is for one moment illuminated by a hundred brilliant points, and the next is in almost total darkness. The intervals have about the duration of a second, and during the intermission only one or two remain luminous.

Burbidge (1880) also describes a display near Singapore: "As we glided onwards their numbers increased, until we came upon them in thousands, evidently attracted by some particular kind of low tree, around which they flashed simultaneously, their scintillating brilliancy being far beyond what I could have imagined to be possible."

Severn (1881), referring to an Indian firefly, says:

The curious pulsation of flashing of their light is remarkable, the insects on the tree all act in perfect concert, i.e., five seconds of no light, then seven rapid flashes; five seconds no light, seven flashes; and so the game continues throughout the dark hours. It is also worthy of special notice that all the glow-insects on a dozen or more trees will continue to keep up the most perfect time to the flashing of their light and the interval of pause, and this for many consecutive hours; but this singular agreement as to the time relates to close clusters of trees only. Thus distinct groups of trees separated by one or more hundred yards may not agree, and do not do so as a rule.

Annandale (1900) has given us a rather detailed and careful description of a display witnessed in Malaya:

A large tree was covered with many hundreds of fireflies, the majority of which seemed, judging from the similarity of their lights, to belong to one species, or perhaps to one sex. There were three individuals seated together, however, whose lights were larger and bluer than those of the others. The lights of all the specimens of the more abundant variety flickered in unison with one another; those of the minority, the three individuals, flickered together also, but in different time. At one instant the tree was lighted up as if by hundreds of little electric lamps; at the next it was in complete darkness except for three blue points.

Cunningham (1907) says, in reference to Bengalese fireflies:

As the train panted slowly upwards, many of the trees alternately flamed out into dazzling splendor and vanished off in the gathering gloom of an impending storm, whilst hosts of insects resting in them lit and put out their lamps as though by common consent. The cause for such simultaneous action on the part of countless individuals is hard to imagine, but there can be no question of the fact that such displays do take place.

Shelford (1916), referring to a display in Borneo, says: "On the opposite bank was a small tree growing close by the water's edge, which was covered with thousands of Fire-Flies, small beetles of the family *Lampyridae*; and I observed that the light emitted by these little creatures pulsated in a regular synchronous rhythm, so that at one moment the tree would be one blaze of light, whilst at another the light would be dim and uncertain."

Wells (1924, 1925) describes a rather astonishing manifestation of synchronism in Malaya, although it is not clear whether this display involved rhythm also. He says:

About every fifteen minutes these flies separated into two armies, one settling on the trees growing on the left bank of the river and the other on the right bank. Then, when I had decided that they had gone to bed for the night, the whole army on the left bank gave one big flash in perfect unison, which was immediately answered by another big flash from the right bank. There must have been thousands of them stretching along the river bank for a hundred yards or more, but the flies at one end of the line flashed their lights exactly at the same time as the flies on the other end. . .

Guenther (1931) reports witnessing a display in Brazil. He says: "In Petropolis . . . I noted how hundreds of green lights blazed out *simultaneously* and were simultaneously extinguished; with so regular a rhythm that it seemed as though the sparks were blown upon by a huge

mechanical bellows that gave a puff every second."

To pass more cursorily over the more accessible recent reports, rhythmic synchronous flashing of fireflies has been observed in the Philippines by Cox (1917), Purssell (1918), Barnes (1919), F. Morse (1918), and Brokenshire (1929); in New Guinea by Dodd (1918); in Siam by Reinking (1921), Morrison (1927, 1929), Smith (1935), and Alexander (1935); in Jamaica by Miller (1935); in India by Connor (1933); in Mexico by Merril (1930); in Borneo by Muir (1908, 1916); and in this country by E. S. Morse (1916), Allard (1916), Hudson (1918), Snyder and Snyder (1920), Hess (1920), and Rau (1932). In addition, there have appeared a considerable number of more or less circumstantial second-hand reports from various parts of the world. Some of these are cited in the reviews of Blair (1915), E. S. Morse (1916 a, 1916 b, 1918, 1924), Craig (1916), Allard (1917), Dahlgren (1917), Gudger (1919), Williams (1922), Allee (1927, 1931), Howard (1929), and Crawford (1934).

Of the 36 reports of synchronous flashing discussed above, 15 are from the Malay Peninsula (Siam, Burma, Malay States, etc.), 5 from the Philippines, 3 from Bengal, 3 from the East Indies, 6 from the United States, 2 from Brazil, and one each from Mexico and Jamaica. The preponderance of reports from the Oriental region (26) as compared with those from the rest of the world, is very striking.

The evidence presented leads to the following conclusions:

(1) It is reasonably certain that rhythmic synchronous flashing of fireflies occurs.

(2) Nearly all the Oriental reports state that the synchronism involved huge numbers of insects; that the displays occurred on trees growing in or near water; that synchronism is common in some regions and species; and that a given display persists unbroken sometimes for hours. The reports from the United States, markedly fewer and less consistent than those from the Orient, give the impression that synchronous flashing is exceedingly rare.

(3) In some displays observed the synchronism was reported to be "perfect," i.e. all the insects flashed at precisely the same instant (Kaempfer, Goldsmith, Bowring, Burbidge, Annandale, E. S. Morse (1916 a), F. Morse, Reinking, Wells, Morrison, Brokenshire, and Smith). In others the synchronism was not perfect: it either began at one or several foci among the aggregated insects and swept rapidly over the whole assemblage ("wave synchronism," Hudson, Hess, Williams, and Alexander), or a few insects flashed in the "dark" periods between mass synchronous flashes (Collingwood, Annandale, Purssell, Barnes, Hess, Miller, and Smith). Annandale, Hess, and Smith, however, maintained that the asynchronous flashes were produced by individuals of a different species, and Morrison suggested that they were due to the residual glow of the light organ after the synchronous flashes.

II. INTERPRETATIONS

The theories propounded to explain synchronous flashing are nearly as numerous and remarkable as the reports of the displays themselves. Some of these theories deal with synchronism, some with rhythm, and some with both. It will be convenient to separate them into five groups.

Obviously inadequate explanations

Included under this heading are the suggestions that puffs of wind influence

the insects alternately to expose and conceal their lights (*Trans. Ent. Soc. Lond.*, 1865); that synchronism is due to twitching eyelids (Laurent, 1917); and that the synchronism is in some way connected with the sap of the trees on which the insects gather (Morrison, 1927).

Accident

The accident theory is based upon the common observation that fireflies tend to flash rhythmically and that the rhythm is of approximately the same frequency in all the individuals in a given region. The argument then is that if the fireflies once get into synchronism with one another they will continue to flash synchronously because of their equal rhythmicities. The supporters of the theory are thus concerned with suggesting ways by which the original synchronization might accidentally be brought about. Gates (1917), however, apparently believed not only that the first synchronous flash occurred entirely by accident, but, what seems statistically even more improbable, that each succeeding rhythmic synchronous flash also was accidental.

Several authors (Allard, 1916; Hudson, 1918; Rau, 1932) have noted that some displays of synchronism take place under unusual environmental conditions, such as exceptional humidity, calm, darkness, etc., and in a large open space crowded with insects. Snyder and Snyder (1920) maintain that such conditions insure the uniformity of the natural rhythmicity of flashing so that synchronism, once initiated, will continue. No mechanism for initiating synchronism, however, is suggested. McDermott (1916) and Rau (1932) similarly regard synchronous flashing as a rare phenomenon brought on by a fortuitous concatenation of atmospheric factors. Rau offers, for the display he observed, the following tentative explanation: "Perhaps the entire population was ready to rise in flight but was held back by the shower; when this suddenly ceased, they were all in equal readiness, and at the propitious moment took to wing and flashed together. Since their flashes are at a fairly uniform interval, they continued in unison quite by accident, and did not break step for a few minutes."

The "accident" theory would account for "perfect" synchronism, but not for "wave" synchronism. It should be noted, however, that the definition of synchronism is an academic question: The percentage of a population which must be performing a rhythmic action simultaneously in order for it to be called synchronous doubtless varies greatly depending on the type of behavior and the frequency of the rhythm. "Wave" synchronism, however, is fundamentally incompatible with "perfect" synchronism, since it seems clear the the former must involve a response of each individual to the flash of a near neighbor, so that the community flash occurs progressively, like an ignited gunpowder train, and it is equally clear that the latter could not be brought about by such a mechanism.

Whereas special environmental conditions might conceivably induce isolated instances of synchronism, it seems unlikely that they can account for the spectacular displays exhibited by Siamese fireflies, in which Smith (1935) asseverates that the synchronism continues hour after hour each night for months, "without regard to air currents, air temperature, moisture or any of the other meteorologic conditions which have been stated to influence firefly flashing." Moreover, it is open to question whether the basis of the accident theory, namely, that the synchronism, once initiated, would continue autonomously, is tenable. Thus Snyder and Snyder (1920)

and Buck (1937c) have shown in an American species that there is a normal individual variation of from five to ten per cent in the mean duration of the rhythmic interval between successive flashes, even under uniform conditions, so that these individual variations would in all probability very soon carry the various individuals entirely out of phase with one another and disrupt the synchronism.

Illusion

Bates (1865) first suggested that synchronous flashing is an illusion. This notion has been strongly championed on psychological grounds by Craig (1916, 1917) and Ruckmick (1920). Craig contends that all synchronous rhythmic activities depend on a (perhaps unconscious) learned "conceptual awareness of the relation between one's own actions and the actions of others, and purposive imitation of the latter." This faculty, he says, is present only in man, although lower animals may exhibit an admirable synchronism (not rhythmic), which, under special conditions, may coincide with some powerful environmental rhythm. Craig then argues that the reports of synchronous flashing are illusions due to a "predisposition to perceive rhythm" in the accidental synchronism statistically to be expected in aggregations of flashing fireflies.

This thesis of illusion is amplified from the standpoint of experimental psychology in a remarkable contribution by Ruckmick (1920). From a study of the published reports Ruckmick concluded that most of the observations were made in an emotional attitude "bordering on the romantic" and that their accuracy was thus subject to suspicion. Supposing, then, that several coincidences should take place in a large number of flashing fireflies, the "well-known tendency of the human mind to integrate its experiences" would, according to Ruckmick, "tend to set the mind of the observer in the direction of subsequent groupings of the flashes into patterns, supplied, for the most part, by himself." Ruckmick accordingly tested this hypothesis in the darkroom, by exposing subjects to numerous electric lights flashed by machinery in a disorganized manner (no pattern or rhythm). These experiments resulted, first, in spontaneous (unsolicited) comments that the flashing lights reminded the subjects of fireflies flashing, and second, in likewise unprompted groupings of the flashes into spatial, and more especially, temporal patterns, the latter being described as "rhythmic." Ruckmick's conclusion is that under conditions simultating the natural there is a strong tendency for the mind to read order into disorganized experiences, and on this account reports of prolonged synchronous flashing are likely to be unreliable.

It seems unlikely that the evidence and arguments advanced by Craig and Ruckmick, cogent as they are, can serve to invalidate the multitudinous reports of synchronous flashing, many of which were made by experienced and careful observers, but at any rate there are at least two questionable features in Ruckmick's work: (1) His premise that not infrequently all the fireflies in a given large group would accidentally flash in unison is statistically very improbable. (2) Ruckmick's experimental setup was not an accurate imitation of the flashing of fireflies in nature, in that his lamps were not flashed rhythmically; whereas actually most of the fireflies in a given region flash rhythmically and with very nearly the same period (Snyder and Snyder, 1920; Buck, 1937 c). Moreover, the present writer was unable to detect any synchronism in the flashing of huge ag-

gregations of tropical fireflies, in which each individual flashed with the same rhythm, but quite at random with respect to the others (Buck, 1937 b).

Sense of rhythm

Bowring (1857), Theobald (1866), and Wheeler (1917) consider the synchronous flashing to be due to a "sympathy" or "Einfühlung" among the insects. This principle apparently corresponds to the "conceptual awareness" of Craig, already discussed, and to the "organic law of rhythmic appreciation" of Allard (1935). This idea involves consciousness of one's own rhythm, consciousness of one's neighbor's rhythm, and deliberate imitation of the latter; or, in anthropomorphic terms, a "sense of rhythm."

The "sense of rhythm" or "sympathy" hypothesis is not so much an attempted explanation of synchronous flashing as it is a more or less gratuitous attempt to relate the phenomenon to similar behavior in higher animals and man. The reason for this attempt is, apparently, the conviction that displays of synchronism in which all the fireflies flash exactly in unison cannot be understood without postulating the existence in insects of some faculty similar to that which enables human beings to walk or sing in unison. It is, however, possible to explain such apparently exact synchronism in a simpler and more objective manner. Such an explanation is discussed below.

Leader or pacemaker

The most popular proposed explanation of rhythmic synchronism is the "leader" or "pacemaker" theory, espoused by Blair (1915), Allard (1917, 1918), Hudson (1918), Hess (1920), and Alexander (1935). The essential feature of this theory is that all the fireflies simultaneously respond to the flash of one (the leader), which flashes rhythmically. This theory is an improvement over the accident theory in that it provides a mechanism for keeping the flashes synchronous so that the normal individual variations in rhythm will not break up the synchronism, but it is nevertheless open to criticism on the following points:

(1) Most of the proponents of the leader theory maintain that the flashes take the form of a wave, whereas a communal response to a single leader would necessarily result in perfect synchronism.

(2) As pointed out by Annandale and by Morrison, a single leader could not possibly be visible to all of the members of such huge swarms of fireflies as are reported to exhibit synchronism. (The fact that the compound eyes of insects are situated on the sides of the head, and designed for perception over a very wide angle, makes this a less serious objection than it would be if applied to primates, where the field of view is greatly circumscribed. Furthermore, since so many of the reports emphasize the wetness of the localities in which the displays of synchronous flashing occur, it might be imagined that the flash of the leader could be relayed to the rest of the swarm by means of its reflection on the water below.)

(3) No mention is made of what determines which individual shall be the leader, or of why he should flash more regularly than the others. Unless the leader's flash is somehow different from all the others (of which there is no evidence) he would have to flash at a slightly faster rate than any of the other fireflies to prevent being anticipated by some individual which happened to flash a little sooner than the average rhythm interval. Moreover, if each collective flash depended on the leadership of a single individual the whole group would re-

main dark if the pacemaker missed a single beat.

(4) The contention of Alexander (1935) that "in the absence of a pacemaker mechanism we should be forced to postulate the existence of an accurate physiological chronometer, a mechanism to most of us quite inconceivable," seems scarcely germane, since it is precisely such a mechanism which must activate the "pacemaker" itself. Moreover, not only are we quite familiar with such chronometers, or their manifestations in higher animals (e.g., heartbeat, action potentials in nerve, peristalsis, respiratory rhythm, etc.; cf. Child [1924] pp. 184–186) but there is good reason to believe, from the work of Brown and King (1931), Snell (1931, 1932), and others, that such a mechanism exists in the firefly. Snyder and Snyder (1920), indeed, have compared the flashing rhythm to the rhythmic electrical discharges originating in the central nervous system of vertebrates. Further data on the physiology of pacemakers are contained in Hoagland's (1935) book.

Richmond (1930) has suggested a simplified form of the leader theory, which, because it eliminates the idea of a single fixed pacemaker, obviates some of the criticisms presented above. Richmond's theory postulates first, that all the fireflies flash rhythmically, with nearly the same period, due to the alternate discharge and recovery of some battery-like mechanism (cf. also Blair), and second, that each insect flashes immediately if stimulated by a flash which occurs at a time near that at which he would normally flash. The title "leader" thus would apply merely to the firefly which happened to flash first after the last concerted flash.

Richmond's postulation is supported by certain analogies in nerve physiology. If, for example, the flashing is under nervous control (which seems certain from the work of Lund [1911], Brown and King, Snell, *et al.*), it might be expected that following each flash there would be a "refractory" period during which the insect would not respond to the stimulus of a flash, and during which he would not flash of his own accord (inhibition?). After this period a potential of some sort would be built up which would result ultimately in a spontaneous flash, but which could be discharged meanwhile by the stimulus of a flash from some other individual (cf. Allee, 1931, p. 92, and Child, 1924, pp. 276–287). Certain experiments of Hess (1920) and Buck (1937 c), described below, indicate that a mechanism similar to that here postulated may be present in the firefly, although Snyder and Snyder assume that at a given temperature "the flashing mechanism discharges [spontaneously] as fast as it can." That the battery analogy is not an accurate one in the sense that it implies that some substance is used up during the flash, and another flash cannot occur until more substance appears, is indicated by the work of several investigators who induced fireflies to flash at a rate higher than normal by electrical stimulation, or treatment with chemicals.

Summary

It will be seen that none of the theories proposed to account for synchronous flashing of fireflies provides an adequate explanation of the phenomenon. Richmond's modified leader theory appears in general to present the fewest intrinsic difficulties, although, like the other theories, it is mainly speculative. It provides a reasonable explanation for perfect synchronism within a relatively small area, by postulating that all the fireflies in the area respond synchronously to the first individual within sight which flashes after the last preceding synchro-

nous flash. It also can be adapted to explain synchronism over a larger area, providing that each synchronous flash passes as a wave over the assembled fireflies, by postulating that each firefly responds not to one particular individual, but to the flash of some member of the swarm, and that he, in turn, stimulates another, and so on.

Richmond's theory, however, like all the other theories, appears to be completely inadequate to explain displays of synchronism of the magnitude observed in Siam by Morrison (1927, 1929) and Smith (1935), who report that enormous numbers of fireflies, distributed along a half-mile of river front, all flash in exact unison. If the synchronism is really as absolute as reported, there seems to be no obvious escape from the necessity of referring the phenomenon to some abstract faculty like "sense of rhythm," since all the individuals obviously could not be responding to a single leader. However, the frequency of the flashing rhythm is reported to be very high (more than a hundred times per minute, according to several observers), and this ought to make it very difficult to distinguish between absolute synchronism and a very rapid wave flash of the type described by Alexander (1935), presumably in the same fireflies studied by Morrison and Smith. Assuming that the synchronism is in fact of the wave type, the Siamese displays are amenable to a simpler explanation than the above, by means of the Richmond leader theory. The reaction time between the flash of the leader and the response of the other fireflies, however, would have to be exceedingly short.

III. EXPERIMENTAL

Introduction

Gates (1917) was unsuccessful in attempts to produce synchronism or to alter the frequency or manner of flashing in *Photuris pennsylvanica* by means of a flashlight.

Morrison (1927, 1929) states that he inhibited the synchronous flashing of fireflies in a tree "by exposing them to a bright light for about a minute," and that "when the light is turned off the synchronism returns, having its origin apparently in some individual or group generally located in the central part of the tree."

Hess (1920) maintains that in observations on synchronous flashing in *Photinus consanguineus* he discovered that "by standing on the side of the valley and causing short flashes with a pocket flashlight, the fireflies of the entire valley responded," and that by flashing slightly sooner than the normal time for flashing he could increase the frequency of the synchronous flashes to some extent.

Allard (1935) induced several females of *Photinus scintillans* to respond synchronously to flashes from a flashlight. This has been observed many times by the present writer in females of *Photinus pyralis*, but since in both species the responses occur only following a stimulus they are not truly rhythmic.

Relation between the mating signals and synchronous flashing

It has been demonstrated (Buck, 1937 c) that the male and the female of the American firefly, *Photinus pyralis* are brought together for mating by a system of flashing signals of which the essential feature is that the responding female invariably flashes two seconds after each successive rhythmic flash of the male.

Earlier (Buck, 1935) it was shown that during the mating signals several males are often attracted simultaneously to the same female, and that these males flash in unison. It was further shown that synchronism on a larger scale can readily be induced by substituting a flashlight for

the female in the mating signals, and a theory to explain how such synchronism could spread to include a large number of fireflies was presented, as follows: "A little group of synchronously responding males built up around one female, as described above, acts as a unit in stimulating another female a considerable distance away because the combined intensity of the several simultaneous flashes is greater than that of a single flash; the second female, then, in responding to the first group of males, gathers in to herself a coterie of males which flash in unison and are, of course, synchronized with the original group; they in turn stimulate a third female which "attracts" a third cluster of males, also synchronized with the original, and so on, until a large number of fireflies scattered over an extensive area are flashing in unison. The whole process depends on the fact that all the females reply to each of the flashes of the male at the same definite interval."

This theory is a reasonable explanation of synchronism in *P. pyralis*, but does not account for the synchronous flashing observed by Hess (1920) in *P. consanguineas*, if his contention that both males and females flash synchronously together is valid. In addition, Miller (1935) and Smith (1935) assert that it does not account for the displays which they saw in Jamaica and Siam, respectively, because no females were seen. Smith in fact maintains that the females remain in the jungle from which the male fireflies "have definitely flown for the purpose of engaging in this nightly display."

Relation between subjection to darkness and synchronous flashing in P. pyralis

Males of *P. pyralis* were put into a large mosquito-netting cage in the darkroom and subjected to a light intensity of about 65 meter candles (for details see Buck, 1937 a). It was found that sooner or later, depending on previous treatment, these fireflies began to fly about and flash in a manner typical of their behavior in the field. Under these circumstances, their flashes, although rhythmic for a given individual, showed no sign of synchronism. If these males were then suddenly subjected to total darkness the character of the flashing was immediately and strikingly altered. All the fireflies flashed synchronously: then this fulguration was followed by four or five seconds of complete darkness, after which another salvo of flashes ensued. This rhythmic synchronism persisted uninterrupted for a considerable time and then gradually died away. Careful observation showed that each community flash was initiated by a single flash, the others following almost instantaneously. Each individual apparently took his cue to flash from his more immediate neighbors, so that the mass flash took the form of a very rapid chain of overlapping flashes, the insect farthest from the "leader" flashing last, but so promptly as usually to light up before the leader's flash had faded. That the synchronism was truly due to a follow-the-leader system and not to accident or individual adherence to some fixed rhythm is shown by the following facts: (1) Different individuals acted successively as the leader, the first flash after the dark between period being the one followed, regardless of its source; (2) There was considerable variation in the time between successive concerted flashes during the later, more irregular phases of the process, in which as much as ten seconds sometimes elapsed before the signal for flashing was given. That the synchronism was not induced specifically by the unnatural conditions of complete darkness is indicated by the fact that the same phenomenon was occasionally seen in the field among males which were sufficiently close together. The

total darkness thus seemed to serve only to increase the relative intensity of the flashes of the neighbors.

This synchronizing tendency in the males of *P. pyralis* probably could not in itself produce a mass synchronism among large numbers of specimens in the field, such as the display reported by Rau (1932), unless the insects were extremely crowded, and then the synchronism would be expected to take the form of waves spreading out from one or several foci. However, it may be the explanation of how several males which respond to the same female get into synchronism with each other (Buck, 1935).

The responses of the male of *P. pyralis* thus appear to be of two types: (1) To a stimulus occurring approximately two seconds after his flash he responds by orienting and proceeding toward the lure, whether female, flashlight, or another male (Buck, 1937 c). (2) To flashes which are close enough to him, i.e. of sufficient relative intensity, and which occur near the time at which he would be expected to flash, he responds by flashing synchronously with them (cf. Richmond, 1930).

IV. SUMMARY

The evidence presented in the preceding pages indicates clearly that rhythmic synchronous flashing of fireflies is a very complex phenomenon, and that it differs greatly in different localities and species.

The four principal theories formulated to explain synchronous flashing are analyzed in detail: (1) The "accident" theory, which postulates that synchronism is initiated by accident, and maintained by the normal rhythm of flashing of the members of the swarm. (2) The "illusion" theory, which holds that the human mind deceives itself into seeing rhythmic synchronism in the few accidental coincidences which would (allegedly) be expected to occur in large numbers of fireflies flashing at random. (3) The "sympathy" theory, which attempts to relate synchronous flashing to the human "sense of rhythm." (4) The "leader" theory, which maintains that synchronism is produced by the mass of fireflies responding to the flash of a leader.

None of these theories is adequate to explain fully all the features of synchronous flashing observed. In general, a modified form of the "leader" theory appears to be the most satisfactory.

Experimental evidence is presented which indicates that in the American firefly, *Photinus pyralis*, synchronism is sometimes a by-product of the flashing signals used in mating, and is sometimes due to a follow-the-leader response.

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SOCIAL FACTORS IN THE ORIGIN OF DARWINISM

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THE promulgation of the doctrine of evolution in the publication of *The Origin of Species* was a profoundly important event not merely for biology but for society as a whole. The social implications of Darwin's work concerning, for example, the origin of man or the doctrines of *laissez faire* and "progress," were disturbingly pertinent to the fundamental religious and socio-economic issues of mid-19th century life. Indeed, the acceptance of *The Origin of Species*, even in the biological world, was expedited by the relevance of the subject of evolution to the general intellectual temper of the day.

This impact of evolutionary thought on 19th century culture is an oft-told story. But it does not seem to be as generally recognized that the origin of Darwinism was in many ways an outgrowth of social conditions similar to those it affected. It is the purpose of this essay to discuss certain aspects of this relation, to trace the influence of some of the more important non-biological social factors on the maturation of the doctrines of evolution into what became known as Darwinism.

I

Darwinism is here understood to mean the body of fact and theory bearing on the general problem of the origin of species presented by Charles Darwin in his works *The Origin of Species* and *The Variations of Animals and Plants under Domestication*. It is in these publications that we find the first extensive proof that organic evolution is a fact, and that natural selection is the mechanism of evolution. In tracing Darwinism, in this sense, to its social roots we are dealing with a problem of contemporary interest. For Darwinism is still one of the most outstanding and fundamental truths of biology; and it is generally admitted to-day that the outline of evolution as given in the above mentioned works still holds. Furthermore, Darwin's work included so many fundamental divisions of biology that are still being actively investigated, that a study of its social origins is in considerable measure a study of social factors that affected the development of the whole of modern biology.

Darwin developed his proof that species originate by evolution from the facts of natural history, distribution, comparative anatomy, geology and paleontology, and variation and selection. His apprehension of natural selection as the mechanism of evolution was based on a reading of Malthus' book on the principle of population. The task of demonstrating social origins in Darwinism therefore reduces to revealing the influence of relevant social factors in the development of natural history, distribution, comparative anatomy, geology and palaeontology, variation and selection, and Malthusianism.

II

The first three of these subjects form a rather well-defined unit, for they grew together as the knowledge of organisms

was extended to include species from all parts of the world. The problem of social influences in the growth of this unit is so extensive that it can be treated here only in broad outline. At the close of the Dark Ages (*ca.* 1000) the formal knowledge of organisms was conditioned by ecclesiastical limitations; there was a widespread and naive faith in the dogma of the universality and eternity of the commonly known species, mostly European, and in the existence of a host of mythological creatures; and there was a blind acceptance of the story of the special creation of all living things by Divine Fiat given in the *Book of Genesis*. These beliefs were gradually broken down as new information about plants and animals poured into European centers with the return of travelers to distant lands.

The first of these movements of major significance was the Crusades (11th–13th centuries). These expeditions were only in part motivated by religious zeal. Quoting H. G. Wells,

> There was the cold and calculated scheme of the free and ambitious Latin Church to subdue and replace the emperor-ruled Byzantine Church; there was the free-booting instinct of the Normans . . . which turned readily to a new and richer world of plunder; . . . the intolerant Seljuks and the intolerant Fatimites lay now an impassable barrier across the eastward trade of Genoa and Venice that had hitherto flowed from Bagdad and Aleppo, or through Egypt. They must force open these closed channels, unless Constantinople and the Black Sea route were to monopolize Eastern trade altogether. . . .

While the forces that determined the Crusades were not put into motion by the desire to develop biology, the incidental biological results were notable. Contacts between the West and the East were reestablished; the biological works of Hippocrates, Aristotle, and Galen, in the original Greek, preserved and annotated by the Moslems, were introduced to Europe; new observations and comparisons of Asiatic and European organisms were made. Here were multitudes of new facts of natural history bearing on the yet undeveloped sciences of comparative anatomy and distribution; here was a fresh ferment that began the disintegration of the old biology and the formation of the new.

These activities in the growth of modern biology were greatly extended and accelerated by the later explorations of the 15th and 16th centuries. The primary aim of these voyages was the discovery of a new trade route to the Far East. While the achievement of such a route (Vasco da Gama's voyage, 1497–9 and Magellan's voyage, 1519–22) was of minor importance for trade, the discovery of America and its rich deposits of precious metals and other natural resources held by relatively undeveloped and practically defenseless natives, was highly significant; the desire to penetrate and exploit the new lands became a powerful impetus to a new wave of commercial expansion.

Incidentally biology was enriched. Says Miall, ". . . the discovery of a new world across the Atlantic immediately created a thirst for selfish acquisition accompanied by a far weaker but nevertheless invaluable impulse to learn all that could be learned about the new strange lands." This impulse to learn was, for biologists, particularly whetted by the reports of the entirely novel inhabitants of the Americas, and of animals and plants not listed or described by ancient or contemporary naturalists. This was especially true for the organisms of the West Indies, Brazil and Mexico, all explored by the Spanish; for, while North American species, as we now know, belong to genera or families which occur in Europe or temperate Asia, most of Central and South America comprise a far more peculiar flora and fauna.

Columbus on his first return in 1493 exhibited at the Spanish court Indians and parrots, and it was not long before Europe for the first time began to hear of the sloth, opposum, tapir, ant-eater, manatee, and humming-bird; the puma, llama, and alpaca; maize, cassava, pineapple, and prickly pear. Books describing the new natural history soon began to appear (See Miall, 1912). Peter Martyr Anglerius, chief chronicler of Indian affairs to Ferdinand and Isabella, published his *Decades* which was the chief source of information on the Americas to Europeans during the early 16th century. A crude attempt at, and what is more the need for, a science of comparative anatomy, is evident in his quaint description of the opposum, the chief oddity of the new fauna. He states it to have the snout of a fox, the tail of a monkey, the ears of a bat, the hands of a man, and the feet of an ape! A more definite biology of the new lands is to be found in the *General and Natural History of the Indies* (1526–1535) by Gonzalo Fernandez de Oviedo y Valdes (1478–1557), a mining inspector in America.

The effect of the new knowledge on the validity of the Biblical account of creation is thoroughly discussed in the *Natural and Moral History of the Indies* (1588) by the Jesuit Father Joseph de Acosta (1540 ?–1600) who did service in Peru. Acosta, it is true, adheres to the story of the *Book of Genesis*, but his work shows an awareness of the new problems of distribution raised by the discovery of America and attempts their solution by the hypothesis that New World organisms migrated from the Old World by way of a land bridge between the continents somewhere near the North Pole. Be that as it may, Acosta's reasoning vividly illustrates how the exploration of the New World led to results that weakened belief in the Biblical account of special creation by forcing the validity of this doctrine to depend on the assumption of adventitious hypotheses.

Contemporaneously with the opening up of the Americas, somewhat similar explorations were bringing Europeans into contact with the African coastline and tropical Asia. Colonization in these lands, however, was much more difficult than in the New World, due to the extreme distances of travel, the excessive heat of the East Indies, and the resistance of the more populous and better armed natives. Hence, it was not until much later—the 17th and 18th centuries—that Europeans successfully penetrated some parts of the East. At this time, when colonization had become profitable, the Dutch began to publish methodical treatises of the natural history of India and the Malay Archipelago.

The period from the late 15th to the 18th century thus was a time of widespread exploration primarily concerned with political and economic expansion. The discovery and colonization of the new lands led to quite unanticipated biological results—the accumulation in Europe of vast numbers of new facts of natural history. And it is significant for our thesis to note that this accumulation was, at first, not effected by biologists, but by those directly concerned with the more fundamental social aspects of the conquest of the new lands; by Columbus, the explorer; Anglerius, the court chronicler; Oviedo, the mining inspector; and Acosta, the missionary. Nevertheless, a new biological world had been opened—more extensive than the old and often at variance with it.

It was long before the new material was comprehensively appreciated. Says Miall,

> The scientific gain which accrued from the multitude of new species was not really so great as it appeared to be. So vast and sudden an accession of

facts overpowered rather than strengthened the infant studies of zoologists and botanists. . . . Europe was as ill-prepared to grasp the new opportunities of enlarging the knowledge of terrestrial life as politically and morally ill-prepared to use her conquests in Mexico and Peru to the lasting advantage of mankind. . . . It was not till the Age of Buffon that comprehensive and daring questions were raised in earnest, and that the new sciences of geology and paleontology began to enforce the pregnant thought that facts unintelligible on the theory of sudden creation might receive an explanation from long-continued development. . . .

And, as we shall soon see, this contributing rôle of geology and paleontology in the coming of evolution, was itself determined by underlying social factors.

The 19th century witnessed a new period of exploration related to the consolidation of the earlier conquests into the colonial empires of contemporary European nations. These voyages also led to an incidental biological counterpart which, in reflection, as it were, of its politico-economic mediation, was chiefly concerned with the consolidation of its facts into an integrated theoretical system.

Among the 19th century explorations, the voyage of H.M.S. *Beagle* (1831–1835) carrying the young Charles Darwin as naturalist was for the history of biology an obviously outstanding event. It is not uncommon to find that present-day biologists are of the opinion that the voyage of the *Beagle* was primarily a biological expedition. This view is erroneous. Darwin himself states in the opening paragraph of his book *The Voyage of the Beagle* that,

> The object of the expedition was to complete the survey of Patagonia and Tierra del Fuego, commenced under Captain King in 1826 to 1830—to survey the shores of Chile, Peru, and of some islands in the Pacific—and to carry a chain of chronometrical measurements around the world.

The position of Darwin as a dependent supernumerary on board the *Beagle* is clearly expressed in the following quotation from the preface of the same work,

> . . . it was in consequence of a wish expressed by Captain Fitz Roy, of having some scientific person on board, *accompanied by an offer from him of giving up part of his own accommodations*, that I volunteered my services which received, through the kindness of the hydrographer, Captain Beaufort, the sanction of the Lords of the Admiralty. *As I feel that the opportunities which I enjoyed of studying the Natural History of the different countries we visited, have been wholly due to Captain Fitz Roy, I hope I may here be permitted to repeat my expression of gratitude to him;* and to add that, during the five years we were together, I received from him the most cordial friendship and steady assistance. (My italics; A. S.)

It is therefore indubitable, to quote Henshaw Ward, that "the work with time and soundings was the chief mission of the *Beagle*; collecting specimens by a naturalist was only a supplementary job."

Nor was this relation between the Beagle and Darwin an isolated instance. The voyage of the Beagle is only one of a series initiated by the British Government for the purpose of making world-wide surveys touching on the possibilities of existence of exploitable natural resources and concerned with the improvement of trade routes, particularly among the far-flung British colonies. As a rule, biological research was not specifically included in the program of activities. But biologists were quick to seize the opportunities offered by these trips to study organisms in their native habitats all over the world. Thus, Thomas Huxley on board H.M.S. *Rattlesnake* and Joseph Dalton Hooker on board H.M.S. *Erebus* were enabled to investigate problems in exotic natural history under circumstances quite similar to Darwin's.

There is no need to go into detail concerning the biological consequences of these explorations. *The Voyage of the Beagle* which exemplifies them has long been a classic in the literature of natural

history; its observations and the rôle that they played in leading the thought of Charles Darwin into evolutionary channels are well known (see Ward). Darwin, himself tells us in his autobiography that, "The Voyage has been the most important event in my whole life. . . . I owe to the voyage the first real training or education of my mind." Certainly it is clear that after Darwin returned to England—his mind packed with observations of the multiplicity and variability of species, of peculiarities of distribution (e.g. in the Galapagos), of details of geology and paleontology, and because of all of these observations believing no longer in special creation—he almost immediately began research in the "mystery of mysteries"—the origin of species. The voyage of H.M.S. *Beagle*, like the Crusades and like the expeditions captained by the Columbuses and the Magellans, while prompted not by biological, but by broad economic forces, nevertheless yielded biological results of decisive importance in the coming of evolution.

III

It has already been stated that social forces were involved in the growth of geology and paleontology. These sister sciences arose in close relation to the development of the mining industry. Let us examine this relation.

There had been geological and paleontological observations as far back as classic Greek times, but they were scattered and unintegrated. Up to even as late as the early 18th century such fossils as had been unearthed were considered to be *lusus naturae*—jokes of nature. But with the development of mining in early modern times fossils in such great numbers were dug up with ores and precious minerals that this view could no longer be maintained. Thus the foundation of fact for the science of paleontology was laid.

Previous to the 18th century there were no sciences of geology and paleontology. In fact, the word fossil during these times did not even have its present meaning. According to Arber, fossil originally meant anything dug up out of the earth, e.g. sands, clays, marbles. During the scientific renaissance a distinction was made between "real or natural" fossils—lithological, petrological, or mineralogical specimens—and "extraneous" fossils, the "fossils" of today. The very terms used emphasize the primacy of mining over paleontology. An exactly similar relation holds in the geological sphere. Wolf states that, "The terms 'geology' and 'geologist' did not come into general use till late in the 18th century, when they gradually displaced the older terms 'mineralogy' and 'mineralogist' . . ." The chief work of this period of interest to the modern geologist is the *De Re Metallica* (1556) by Georgius Agricola (1494–1555)—a classic in the technology of mining engineering (see Wolf). While this work primarily deals with the problems of practical mining, such as surveying, engineering, smelting, assaying, the qualities of competent miners, business administration, there is some discussion of more purely geological matters like stratigraphy, the effects of water and wind, and the origin of ore deposits.

Mining in the 16th century was essentially a surface affair, for the depth to which shafts could be sunk was sharply limited by the capacity of the mechanical contrivances then used to pump water out of the mines. The invention of the steam engine (1765), which played such a dominant rôle in bringing about the Industrial Revolution, particularly revolutionized mining. The new source of power was, in fact, first used to pump water out of

mines; hence its early name, "The Water Commanding Engine" (see Wolf). It was now possible to sink shafts to much greater depths, the mining industry grew apace, and these new conditions were almost immediately reflected in a rapid rate of integrated growth in both paleontology and geology. The practical impetus back of this growth is indicated in the occupations of some of the important geologists of this period: Abraham Gottlob Werner (1750–1817) was professor at the mining academy at Freiberg and, quoting Nordenskiöld, "systematically explored the geology of his own district . . . and on the results thereof based a rational mining-industry . . ."; James Hutton (1726–1797), who published his *Theory of the Earth* in 1785, was a farmer; William Smith (1769–1839), was "a surveyor and leveler" (Nordenskiöld); and Christian Leopold von Buch (1774–1853), was a student under Werner.

The more the mining industry grew, and the more geological problems were investigated, the greater were the number of fossils dug up and studied. The dependence of the development of paleobotany on the mining of coal, for example, is plain to see in the contents, and, for that matter, in the very title, of Arber's paper. The reference to the "British coal measures" is essentially a reference to the British coal mines, and it was the working of these mines that unearthed the fossil plants. As for the growth of paleozoology, Cuvier himself was dependent on a type of mining, as the following quotation from Nordenskiöld shows:

> . . . he soon found himself engaged in a special field of research, the study of fossil forms. As is well known, Paris is situated in the center of a calcareous district in which the stone used for building-material is particularly rich in fossils, . . . it was on the basis of material gathered from this and other districts that he formed his theory of the evolution of the earth and of the creatures living on it.

Mining, and its related industry, quarrying, were thus instrumental in the development of geology and paleontology. These economic pursuits, like exploration, acted as fact-collectors, literally unearthing the facts, and, what is more, as they grew, were faced with practical problems concerning the structure and manipulation of the earth's crust, the solutions of which helped to establish the science of geology. It is a far cry from these rather elementary conditions attending the birth of geology to the grand sweep of Lyell's *Principles of Geology* (1831), but an unbroken development leads from the former to the latter. Lyell's work is of particular interest because it was one of the few books that Charles Darwin studied while on H.M.S. *Beagle*, and it was in this volume that he learned of the principle of uniformitarianism, a doctrine that was destined to play an important part in his investigation of the problem of evolution.

IV

When Darwin returned from the voyage of the *Beagle* in 1835 he was quite convinced that species could not have originated by special creation. Biology had developed to such a state at this time that it was possible, from the facts of natural history, distribution, comparative anatomy, and paleontology, to infer an alternative method—evolution. This, of course, is well known; it has been our interest, however, to demonstrate the rôle of non-biological social processes in helping to establish these branches of biological learning.

Darwin was not content with merely inferring evolution. He hoped to show by observations on living organisms that the species-type was not fixed, that it might suffer change; i.e. he wished to demonstrate evolution in miniature. Furthermore, he longed to discover the

mechanism of evolution. The first problem was successfully investigated by a series of researches on variation and selection of domesticated plants and animals begun in 1837. The second problem was solved by assuming the mechanism of natural selection after reading Malthus in 1838. From these dates until 1859 Darwin painstakingly worked on these two problems, combining the results of his research with knowledge derived from other fields, slowly perfecting his conception of evolution. We will now present the social factors in these two main lines of his work, treating them separately, since they present rather separate social origins.

The research in variation was modelled along the lines of Lyell's principle of uniformitarianism. The general method and results of the problem are clearly stated in Darwin's autobiography:

> After my return to England it appeared to me that by following the example of Lyell in geology, and by collecting all facts which bore in any way on the variation of animals and plants under domestication and nature, some light might perhaps be thrown on the whole subject (of origin of species). My first notebook was opened in July 1837. I worked on true Baconian principles and without any theory collected facts on a wholesale scale, more especially with respect to domesticated productions, by printed enquiries, by conversations with skillful breeders and gardeners and by extensive reading. . . . I soon perceived that selection was the keynote of man's success in making useful races of animals and plants.

Darwin was able to pursue his program of research because of the high level of attainment reached by practical breeding in England in the middle of the 19th century. (See articles in the *Encyclopaedia Britannica*, 14th ed. New York, 1929; Animal Breeding; Robert Bakewell; and under Agricultural Articles—Live Stock and Other Improvements; Capitalist Farming and the Great Landlords.) The improvement of British live stock was begun about 1750. At this time there were no definite breeds; there was great variation among the many local types. Robert Bakewell (1725–1795) in 1760 began the systematic improvement of sheep, oxen, and horses and soon produced the famous breeds of Leicestershire sheep and Dishley cattle. So successful were his methods and so certain the results of breeding from his carefully selected stock, that it was possible for him to establish a trade in ram-letting that netted him a profit of over 1200 guineas in one year from one ram. Artificial selection was entering the arena of commerce.

The business of artificial selection started by Bakewell was extended by others, and to the further improvement not only of live stock, but of domesticated plants as well. The growing needs of the rising English bourgeoisie had created a market for better beef, milk, wool, pork, draught animals, and plant products; and the plant and animal husbandmen, untroubled by any qualms concerning the sanctified fixity of species, but with good business acumen, took advantage of the plasticity of species so that they could improve their commodities and profit from the rising market. In the activities of the artificial breeders Darwin was quick to discern the essential biological basis—the mutability of species. Hence his interest in their business; for if it were accepted that species-types could be changed by man through artificial selection, and the laws of these changes could be discovered, it was but a step following Lyell's uniformitarianism, to apply these findings to the process of evolution in nature—past as well as present.

Darwin not only "collected facts . . . with respect to domesticated productions," and had "conversations with skillful breeders and gardeners," but he became a member of several breeding

clubs—an unheard of association: the scholar, in his search for pure knowledge, mixing with the business-man breeder and fancier—and, moreover, he carried on experiments in selection with ornamental poultry, such as fancy pigeons. His colleagues, who evidently considered Darwin's *metier* to lie in geology, natural history, and taxonomy, as exemplified by his then current work on the structure and classification of the barnacles, must have regarded these breeding excursions with some misgivings. Their attitude is amusingly reflected in the following quotation from a letter of Darwin to Hooker:

> In your letter you wonder what 'ornamental poultry' has to do with Barnacles, but do not flatter yourself that I shall not yet live to finish Barnacles, and then make a fool of myself on the subject of species, under which head ornamental poultry are very interesting.

The results of Darwin's labor on the subject of variation and selection were in part incorporated into his *Origin of Species*, but the complete report did not appear until some ten years after this (1868) when he published his *Variations of Animals and Plants under Domestication*. Our consideration of the manner in which Darwin gathered materials for this work leaves no doubt that it is rooted in the practical activities of plant and animal breeders—activities motivated, not by the desire for knowledge, but for profit. The utilitarian character of this work is incidentally indicated by the titles of books in the advertisements that grace the fly-leaves of the American edition of *Variations* such as: 'The Miniature Fruit Garden,' 'Practical Horticulturist,' 'Draining for Profit,' 'Gardening for Profit.' . . The aura of lucre that enveloped his investigations is well illustrated in Darwin's clinching his argument proving the "force of inheritance" (Chap. XII of *Variations*):

> Some writers (Buckle in History of Civilization; Mr. Bowen, Professor of Moral Philosophy in Proc. American Acad. of Sc. V, p. 101) who have not attended to natural history, have attempted to show that the force of inheritance has been much exaggerated. The breeders of animals would smile at such simplicity; and if they condescended to make any answer might ask: what would be the chance of winning a prize if two inferior animals were mated together? . . . Why have pedigrees been scrupulously kept and published of Shorthorn cattle and more recently of the Hereford breed? . . . Have the Shorthorns, without good reason, been purchased at immense prices, and exported to almost every corner of the globe, a thousand guineas having been given for a bull? . . . With pigs, the Yorkshire and Cumberland breeders 'preserve and print pedigrees' and to show how such highly bred animals are valued, I may mention that Mr. Brown who won all the 1st prizes for small breeds at Birmingham in 1850 sold a young sow and boar of his breed to Lord Ducie for 43 guineas; the sow alone was afterwards sold to Rev. F. Thursby for 65 guineas who writes, 'She paid me very well, having sold her produce for 300 l, and having now four breeding sows from her.' *Hard cash paid down over and over again is an excellent test of inherited superiority*. . . . (My italics: A.S.)

We may well imagine the Reverend preaching of a world of fixity of species on Sundays, but on week-days profiting in "hard cash paid down over and over again" from the changeability of species!

These profit-making origins of Darwin's research, however, did not prevent him from extracting the biological essence, for, in conclusion, he states: "In these laws of inheritance as displayed under domestication we see an ample provision for the production through variability and natural selection of new specific forms."

V

In his autobiography Darwin states, after admitting his indebtedness to "man's success in making useful races of animals and plants": "But how selection could be applied to organisms living in a state of nature remained for some time a mystery to me."

How the mystery was cleared up is given

in the next paragraph of the autobiography:

> In October 1838, i.e., 15 months after I had begun my systematic enquiry I happened to read for amusement 'Malthus on Population'—and being well prepared to appreciate the struggle that everywhere goes on from long continued observation of the habits of plants and animals, it at once struck me that under these circumstances favorable variations would tend to be preserved and unfavorable ones be destroyed. Here then I had at last got a theory by which to work.

The dependence on Malthus may seem to be quite fortuitous. This view, however, is difficult to maintain in view of the fact that Alfred Russel Wallace in 1858 independently propounded natural selection as the mechanism of evolution from a sudden realization of the biological significance of Malthus' work. Wallace in the "Introductory Note" to the second chapter of his 1895 edition of *Natural Selection and Tropical Nature* tells how, in February 1858, he

> . . . was suffering from a rather severe attack of intermittent fever at Ternate in the Moluccas . . . and something led me to think of the positive checks described by Malthus in his 'Essay on Population,' a work I had read several years before, and which had made a deep and permanent impression on my mind. These checks—war, disease, famine, and the like—must, it occurred to me, act on animals as well as on man. Then I thought of the enormously rapid multiplication of animals, causing these checks to be much more effective in them than in the case of man; and while pondering vaguely on this fact there suddenly flashed upon me the *idea* of the survival of the fittest—that the individuals removed by these checks must be on the whole inferior to those that survived. In the two hours that elapsed before my ague fit was over I had thought out almost the whole of the theory, and the same evening I sketched the draft of my paper, and in the two succeeding evenings wrote it out in full, and sent it by the next post to Mr. Darwin.

Darwin's consternation upon receipt of this paper which almost exactly duplicated the yet unpublished results of his twenty years' research on natural selection, and the way in which the suddenly arisen problem of priority was handled by Lyell and Hooker in consultation with Darwin will always remain one of the most dramatic episodes in the history of science. With both Darwin and Wallace, however, admitting their indebtedness to the *Essay on Population*, the conclusion seems inescapable that Malthusianism is directly responsible for the genesis of the theory of natural selection.

To comprehend the social factors of interest here we must trace the origin of Malthus' work. The *Essay on Population* first appeared in 1798 as part of a polemic on the perfectibility of man in which Condorcet, the French metaphysician, and Godwin, the English author and politician, and Malthus, were the principal participants. (Note the extended title of Malthus' book as given in the bibliography.) The social conditions in the last decades of the 18th century that provoked the controversy were in England precipitated by the Industrial Revolution and are well summarized by Wells in *The Outline of History:*

> . . . . The immediate effect of the Industrial Revolution upon the countries to which it came, was to cause a vast, distressful shifting and stirring of the mute, uneducated, leaderless, and now more and more propertyless common population. The small cultivators and peasants, ruined and dislodged by the Enclosure Acts, drifted toward the new manufacturing regions, and there they joined the families of the impoverished and degraded craftsmen in the factories. Great towns of squalid houses came into existence. Nobody seems to have noted clearly what was going on at the time. It is the keynote of 'private enterprise' to mind one's own business, secure the utmost profit and disregard any other consequences. Ugly factories grew up, built as cheaply as possible, to hold as many machines and workers as possible. Around them gathered the streets of workers' homes, built at the cheapest rate, without space, without privacy, barely decent, and let at the utmost rent that could be exacted. These new industrial centres were at first without schools, without churches. . . .

France, for different reasons, had been plunged in revolution during this period.

But there was unrest and the thread of revolt in England too. For example, in 1795, says Bonar: "there had been a serious scarcity; war prices had become famine prices. It was the year when 'the lower orders' were held down by special coercion acts; it was the year when the king's carriage was stopped by a mob crying 'Bread, bread!' "

Whatever the details, it is evident that human society in a good part of Europe in the 1790's was in the grip of a bitter struggle for existence. There was hope in the minds of such utopians as Condorcet and Godwin that the condition of man could be perfected. Malthus, on the contrary, could see only a gloomy future, for he believed that human society was suffering from the inevitable effects of the operation of a natural law,—the tendency of population to increase faster than the means of subsistence—and that as a result of this the poor and the inept, those unable "to get on" in society, must be thinned out by the ruthless agencies of poverty and hunger, vice and crime, pestilence and famine, revolution and war.

It is not our concern to attempt an evaluation of the various solutions offered by the controversialists in this polemic, nor to trace the subsequent breakdown of Malthusianism as a theory of human population. It is sufficient for us to point out that a human struggle for existence, and a theory of the survival of the fittest, are inherent in the Malthusian interpretation of human society, and that the mechanism of natural selection was elaborated in analogy to these supposed conditions of human social existence.

VI

It is now clear that the development of Darwinism was importantly conditioned by a number of precedent non-biological social factors. The Crusades, the exploration and conquest of newly discovered lands, the growth of mining, the business of practical breeding, the complex Malthusian social background, all contributed in one way or another to the elaboration of Darwin's research. Within the short space of this article it has not been possible to trace in full detail the manner in which each of these social factors affected their corresponding biological fields. For example, the relation between exploration and the growth of comparative anatomy has been merely indicated, and it has not been possible to discuss the social conditioning of the growth and failure of Lamarckism—problems which we intend treating in later work.

In considering the relative directness with which the various social conditions affected Darwin in his investigations of the problem of the origin of species it is evident that breeding and Malthusianism take first place. For Darwin himself mingled with the plant and animal breeders; he read their journals; he studied their business at first hand. The relation to Malthusianism is much less extensive but none the less direct. Moreover, the results of these social contacts were of prime importance in scientifically establishing the truth of evolution. For in the results of his examination of artificial selection Darwin found indubitable proof of the plasticity of species; like Lyell in his study of currently operating geological forces, he discovered that the investigation of organic transformations led to conclusions that could be applied to evolution in general. And the Malthusian doctrine of a struggle for existence and the survival of the fittest arising from the contradiction between great reproductive tendencies and relatively little means of subsistence was taken over bodily to form the basis of the theory of natural selection. Although only a coincidence

may be involved, it is interesting to note that artificial selection and natural selection, so straightforwardly related to social conditions, were the biological supports upon which Darwin in *The Origin of Species* leaned heaviest in setting up his proof of evolution.

While the individual social factors we have discussed differ in respect to their weight in influencing the rise of evolution they have in common one very significant basic element: they are all, in essence, economic; they all involve the production and exchange of saleable goods—commodities—or the economic consequences of these activities. Mining, and artificial selection also, as Darwin was so keenly aware, are obviously concerned with the production of commodities; the Crusades, as the excerpt from Wells demonstrates, were strongly motivated by a complex of economic causes; the 15th and 16th century explorations were initially undertaken to improve the exchange of commodities between Spain and Portugal and the Far East, and the unexpected discovery of America, again quoting Miall, "created a thirst for selfish acquisition"; the expeditions of the early 19th century, while carrying on physiographical and chronometric investigations of a high scientific order, were primarily determined by the economic expansion connected with the growth of the present European empires, and, finally, Malthusianism was a direct outgrowth of the social conditions resulting from the Industrial Revolution.

Furthermore, all these economic activities were not unrelated phenomena; they represent various phases of one broad historical movement—the evolution of feudal into capitalist society. Indeed, the development of the very social factors we have been discussing in relation to the genesis of Darwinism in great measure constituted the complex historical flux out of which capitalism crystallized. The earliest emergence of the new society occurred in England (in the very late 18th century), for, in comparison to the rest of Europe, it was this country that manifested the most intensive development of the social activities underlying the change to capitalism. It is this that accounts for the fact that English thinkers of the early 19th century—Spencer, Lyell, Wallace, not to mention Darwin,—played such a predominant and decisive rôle in the coming of evolution. Darwinism sprung up where and when capitalism was most strongly established.

While the economic activities of developing capitalism clearly played a fundamental rôle in the genesis of evolution, the question might still be asked as to whether they were necessary. That is to say—could Darwinism have been produced in the absence of the social factors we have discussed. This, of course, cannot be answered. It is impossible for us to go back and rearrange the course of history in order to see what would have taken place under some different set of precedent conditions. All we can do is to point to the historical development as it—in fact—occurred.

At the end of the beautifully appreciative eulogy, *The Debt of Science to Darwin*, Wallace, paraphrasing the famous couplet of Pope, writes:

> "Nature and Nature's laws lay hid in night;
> God said, 'Let Darwin be,' and all was light."

This is a just tribute to Darwin, and nothing can ever detract from the greatness of the man and his work. In fact it is to his greater glory that he found pabulum for development not only in the purely biological, but also in the basic social phenomena of his age.

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MERRIAM'S LIFE ZONES OF NORTH AMERICA

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OUR present concepts of bioclimatic provinces have had their origin in purely floristic and faunistic studies. With the development of the taxonomy of North American species various distributional patterns appeared, and whenever several of these seemed related they were made the basis of a province. This aspect of biogeography was popular during most of the past century. By 1889, when the Division of Ornithology and Mammalogy of the United States Department of Agriculture undertook its important work in this field, over 50 papers had appeared proposing biotic provinces for North America, about half of them based on floristics and the others on faunistics. Although certain of these pioneer classifications are difficult to coördinate with the others, there was sufficient agreement that in summarizing these papers Merriam (1892) found that North America had been divided into distributional provinces as follows: Arctic; Boreal (subdivided by Allen into Hudsonian and Canadian); Eastern or Atlantic (variously subdivided into Carolinian, Alleghanian, Southern Florida, Austroriparian, Louisianan, etc.); Central; Western or Pacific; and Sonoran (including the closely related Baja California).

Life zone studies by the Division of Ornithology and Mammalogy were begun in the summer of 1889. This venture, suggested by Clinton Hart Merriam, who was then chief of the Division, was prompted largely by economic motives. It was held that a knowledge of life zone boundaries would provide a key to agricultural possibilities which otherwise would have to be determined by the trial and error method for every location.

The first field work consisted of a biological survey of the San Francisco Peak area in Arizona (Merriam, 1890). As a result of observations and collections of the biota with particular emphasis on the altitudinal limits of the species, Merriam, who personally directed the field work, recognized seven major biotic zones: Alpine, Subalpine or Timberline, Hudsonian or Spruce, Canadian or Balsam fir, Neutral or Pine, Piñon, and Desert. Each of these zones was characterized by a group of species not found in the others. Those zones in which the characteristic species were boreal in affinity were grouped into a Boreal Division (including the first 4 zones above), and where the indicators were of southern extraction (the last 3 above) the term Sonoran Division was applied. The fifth zone was called "Neutral" (later referred to as "Transition") because it contained elements of both boreal and austral derivation; however, it was finally classed as Sonoran because of the preponderance of the latter group.

Since certain species of the San Francisco Mountain zones also occurred in the zones previously described for North America, Merriam tried to coördinate his regions with those outlined above. Only his Boreal Division seemed to fit in well with the previous classifications. The integration of these new western findings with

the earlier studies of others was expressed in a map which is remarkably similar to the map of climax plant formations by Weaver and Clements (1929). It is to be noted that this first map, which was probably his best, was based entirely on data of biogeographical distribution.

In this first paper, little attention was given to the causes of the zones. Some of the statements in this regard, however, are significant in the light of future developments. Without indicating the sources of information, Merriam empirically stated that "temperature and humidity are the most important causes governing distribution, and that temperature is more potent than humidity" (*loc. cit.*, p. 26). Previous workers had held that, for birds, the temperatures of the breeding season are more critical than at any other times of the year, so Merriam asks "if this is true of birds, why is it not true of other forms of animal life and of plants as well?" (*loc. cit.*, p. 26). Winter temperatures were considered of little significance since animals either migrate or hibernate or are very hardy, and "freezing does not hurt most plants when not in a state of reproductive activity" (*loc. cit.*, p. 27). It is remarkable to note the diligence with which meager temperature records for various altitudinal zones were compiled, and yet in the discussion of precipitation, no quantitative data were presented, and the altitudinal variation of this factor was ignored.

In the course of subsequent field work (Merriam, 1891, 1893) the preliminary system was modified and expanded to include all of North America, and in 1898 the classification in its final form was published (Merriam, 1898). By this time, many data had been accumulated on the varieties of agricultural plants which could be grown successfully in each zone. A classification of these plants, together with a brief summary of the temperature data on which the explanations of the zones were based, was given in this paper.

The geographic provinces and their approximate distribution were listed in order by Merriam as follows.

The *Boreal Region* included all of the continent from the polar seas southward to the northern boundary of the United States of America, and extended southward along the principal mountain systems. It was subdivided into three zones. The *Arctic-Alpine Zone* was the area above the latitudinal and altitudinal limits of tree growth. The *Hudsonian Zone* took in the northern part of the Canadian forested area, and similar forest zones just below timberline on the mountains farther south. The *Canadian Zone* designated the southern forest region of Canada including parts of the northern tier of the eastern United States, and a belt below the Hudsonian Zone in the mountains.

An *Austrial Region* lying at lower latitudes and altitudes than the preceding was also divided into three zones, each of which in this case was further subdivided into two or three Faunal Areas. These zones were again essentially latitudinal belts distinguished climatically only by temperature, but the subdivisions (the Faunal Areas) were based mainly on humidity. *The Transition Zone* was so named since it seemed to be a region where the ranges of many boreal and austral elements overlapped. It included the Alleghanian Faunal Area (New England to North Dakota, and down the mountains to Georgia), the Arid Transition Faunal Area (the Dakotas, eastern Montana, Manitoba, Alberta, and around the bases of the Western mountains), and the Pacific Coast Transition Faunal Area (the hygrophytic forest region of Washington, Oregon, and California). The *Upper Austral Zone* was divided into a humid eastern

Carolinian Faunal Area (South Dakota to Oklahoma and eastward to Ohio and South Carolina), and an arid western Upper Sonoran Faunal Area (at low elevations west of the 100th meridian). A *Lower Austral Zone* consisted of a Lower Sonoral Faunal Area (west of the 100th meridian, from Texas to the Sacramento Valley and southward into Mexico, taking in the major deserts of North America excepting the Great Basin), an Austroriparian Faunal Area (from eastern Texas and Oklahoma around the Atlantic coastal plain to Virginia), and a Semitropical (or Gulf Strip) Faunal Area (a very narrow coastal strip from southern Texas to Florida, including all of the latter but the southern tip).

The *Tropical Region* extended northward along both coasts of Mexico projecting into the United States of America in the Colorado River Valley and southern Texas. In the east it included the southern third of peninsular Florida.

By way of explaining the distribution of these provinces Merriam stated (*loc. cit.*, p. 54) that "apart from obvious mechanical barriers such as oceans, temperature is the most important single factor in fixing the limits beyond which particular species of animals and plants cannot go. Investigations conducted by the Biological Survey have shown that the northward distribution of terrestrial animals and plants is governed by the sum of positive" mean daily "temperatures," using 6°C as the threshold value, "for the entire season of growth and reproduction, and that the southward distribution is governed by the mean temperature of a brief period" (using the mean normal temperature of the six hottest consecutive weeks) "during the hottest part of the year." He had previously stated (Merriam, 1894) that the southern limits (conditioned by the mean temperature of the hottest part of summer) of the Arctic-Alpine, Hudsonian, Canadian, Transition, and Upper Austral Zones coincide with the isotherms of 10°C, 14°C, 18°C, 22°C, and 26°C respectively. On the other hand, the northern limits of the Transition Zone, Upper Austral Zone, Lower Austral Zone, and Tropical Region correspond with summation isotherms of 5,500°C, 6,400°C, 10,000°C, and 14,500°C respectively. These are all the data ever presented by Merriam in this connection. As shown on maps (*loc. cit.*) the two sets of isotherms practically coincide with each other and with his life zones.

Merriam's work was readily accepted by many biologists. It was indeed a great step forward to utilize both floristics and faunistics as bases for life zones, and at the same time to offer at least a plausible climatic explanation of the provinces. The great accuracy of description of the zones in the western states, and the prestige which the government rendered by sponsoring the investigation as well as by using the system also helped to bring about an immediate acceptance of the classification. Checking up on the explanations of the zones would have been a laborious task. Moreover, it probably seemed futile since Merriam had already availed himself of all the Weather Bureau records. The fact that scarcely any of the fundamental computations were ever published was another impediment. It was easiest to assume that the explanations were sound, and for a time they were never questioned.

In 1902 Adams cast considerable doubt on the concept that distributional patterns conform to latitudinal zones. His maps of species ranges showed them to be related to each other in groups, each of which has a common center of distribution. A few years later Transeau strengthened this latter concept by demonstrating that

centers of distribution are correlated with variation in the precipitation/evaporation ratio. In this paper he expressed his disbelief that distribution could be explained by the temperature factor alone, and suggested the precipitation/evaporation ratio as a method of integrating the most critical factors of plant environment.

During the next two decades the researches of other botanists consistently refuted Merriam's temperature "laws." After an exhaustive study of correlations between the facts of plant distribution and all phases of available climatic data, Livingston and Shreve (1921, p. 528) stated that "the system of life zones worked out by Merriam . . . will require much modification before it may become at all satisfactory to a serious student of etiological plant geography," and eleven years later two lists of criticisms were compiled, this time by zoologists (Kendeigh, 1932; Shelford, 1932). In the following paragraphs is an attempt to summarize the criticisms from all sources.

Destructive criticisms of Merriam's work fall into three categories, viz.: (1) the descriptions of the zones, (2) the explanations of the causal factors, and (3) technical errors.

(1) One of the most significant findings of our active investigations of successional phenomena in the past three decades has been the discovery that the nature of those climax vegetation types which are determined by climate is more significant as an indicator of the totality of climatic influence than are any or all of the climatic instruments yet devised by man. Ecologists now recognize a grassland climate, a summer-green forest climate, a winter-green (monsoon) forest climate, a broad sclerophyll scrub (Mediterranean) climate, etc. If we compare the central grassland province of North America (as mapped by Weaver and Clements, 1929) with Merriam's map, we are immediately struck with the fact that this biotic entity is severed by Merriam into three parts, each of which is linked up by him with one or more distinctly different types of climatic climax vegetation (Transeau, 1905). For example, one of these three segments is included in the Transition Zone along with deciduous forest, ponderosa pine forest, sagebrush, broad sclerophyll scrub, and redwood forest—all climatic climax types. If we follow Merriam in this, we must assume a closer relationship of this grassland segment to these other vegetation types than to the remainder of the grassland! As mentioned above, Merriam's first map of biotic provinces (Merriam, 1890) is in remarkably close agreement with the known distribution of climatic climax vegetation, but the final map was so greatly influenced by instrumental data that it is essentially a climatic map. Thus as a result of using the floristic and faunistic viewpoint (rather than the dynamic sociological one which had not yet been expounded) and emphasizing instrumental data, certain natural entities were artificially split, while very diverse vegetation types were at times lumped together.

Although the zonal descriptions for the mountains where Merriam worked are very good, the lumpings and correlations from region to region are highly objectionable, especially in his Austral Region.

(2) Neither field nor laboratory tests were made to substantiate the temperature hypotheses used as bases for the explanation of distribution. Inferences drawn from very meager studies of a few organisms were assumed to hold true for all forms of life. The same threshold value (6°C) was used for all species of plants and animals. This is obviously invalid when one considers tropic and arctic species, even if older experiments (DeCan-

dolle, as quoted by Kendeigh, 1932) had not already proved it so. The minimum temperature for growth is as low as −2°C for the garden pea, *Pisum sativum*, (Leitch, 1916) and as high as 15–18° for melon and cucumber (Maximov, 1930).

The method of summation used by Merriam is theoretically unsound since each degree of temperature is assumed to have the same significance, and it is now well established that chemical reactions in plants tend to follow Van t'Hoff's Law until temperature high enough to injure the plant is attained (Livingston and Livingston, 1913; Lehenbauer, 1914; Livingston, 1916; Maximov, 1930).

The few detailed studies of temperature summation which have been made in recent years indicate that this idea is of no great significance in connection with plant growth (Seeley, 1917).

It has been pointed out that temperature extremes are much more important for animals than are mean figures for a certain span of summer (Kendeigh, 1932).

Winter temperatures are certainly not to be so completely ignored. Merriam (1894) asserted that low temperatures were significant only in the case of certain plants sensitive to killing frosts. The variation in the ability of plant species to withstand low temperatures is undoubtedly a significant factor in plant geography. Minimal temperatures of winter have been found to have a direct bearing on plant distribution (Shreve, 1911 and 1914; Salmon, 1917; Uphof, 1920; Mason, 1925).

It has been noted that most temperature data give isotherms which roughly parallel latitudinal or altitudinal lines, so that by picking the proper range from any type of temperature data one may get correlation with altitudinal or latitudinal biotic zones (Kendeigh, 1932).

All of these errors have come about through trying to explain the facts of distribution on a single factor of the environment (Transeau, 1905), whereas we now hold the environment to be such an intricate complex of interdependent factors that it is exceedingly difficult, if indeed not an impossibility, to attempt to evaluate the individual influences.

(3) Several important technical errors were made which render the work less scientifically sound. As mentioned above, only the final generalized temperature figures were ever given to substantiate this division of an entire continent into climatic zones. The individual summations on which the isotherms were based were always omitted from the maps. A climax to this obscure procedure came when Merriam himself admitted (1899) that through a misunderstanding with the Weather Bureau, 0°C had been used as a threshold value in the summations rather than 6°C! The agreement of zones with such faulty isotherms is proof of the invalidity of his summation "law" (Kendeigh, 1932).

Finally, it is not the way of the true scientist to subscribe to the complacent attitude expressed in the following paragraph (Merriam, 1895): "It appears, therefore, that in its broader aspects the study of the geographic distribution of life in North America is completed. The primary regions and their subdivisions have been defined and mapped, the problems involved in the control of distribution have been solved, and the laws themselves have been formulated."

In all fairness, however, we must recognize the value of Merriam's life-zone work as a contribution to biology. His was the first major attempt to use climatic data in interpreting the distribution of North American biota, and to base distributional provinces on both plants and animals. It is also to Merriam's credit

that he deplored the use of daily, monthly, or annual climatic data and recommended that the intervals used have less arbitrary bases (Merriam, 1894). Although his explanations may be for the most part fallacious, they have greatly stimulated inquiry into the causes of plant and animal distribution.

The descriptive parts of his field surveys were very accurate and for a long time have been standard references on vegetation zones of western United States of America. Indeed many western biologists, chiefly zoologists, still use Merriam's terminology for the various zones although for the most part they have been replaced by more descriptive terms such as spruce-fir zone, sagebrush zone, etc. And while his map is not acceptable outside of the regions where actual field work was done, it has been useful as a guide which could be verified or disproved by biologists working in restricted portions of the continent.

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ESSAYS ON EVOLUTION

III. ON THE ORIGIN OF INTERSPECIFIC STERILITY

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THE problem of the origin of interspecific sterility has long presented the greatest difficulty in the way of constructing a complete theory of the evolution of species. This difficulty was felt by Darwin, and has, if anything, become more striking with the development of modern genetical theory. One solution, has, it is true, been developed. It now seems clear that, in certain cases, allopolyploidy may be effective in producing a new fertile form that is cross-sterile with its parents. This solution is, however, certainly a special one, applicable only to hermaphroditic organisms. There is no recognized general scheme, applicable to separate-sexed forms, that can be pictured in detail. It is the purpose of the present note to suggest such a general scheme.

It was pointed out by Fisher (1930) that, if a species is supposed to be broken up into two more or less isolated groups, and the hybrids between these two groups are relatively infertile, then any genes tending to decrease the cross-mating between the two groups will be selected. This is because cross-mating, since it leads to the production of less fertile offspring, will lead to a decrease in the potential ultimate reproductive value of the cross-mated individuals, as compared to those that mate within their own group. In other words, intra-group matings will, if we consider generations later than the first one, produce more descendants than will inter-group matings—and selection will therefore decrease the proportion of the latter.

On this basis, sterility of the F_1 comes first, and leads to an accumulation of genes that prevent cross-mating. The alternative assumption, more often made, is that cross-mating is prevented in some manner, and sterility of the F_1 follows as a more or less incidental consequence. This latter view has always remained rather vague, and presents serious logical difficulties when one attempts to make it more precise. There seems to be no good reason why the hybrid should become sterile, yet sterility of the hybrid is one of the most wide-spread characteristics of distinct species.

There is also a logical difficulty about the view that sterility of the F_1 is primary. For, in this case, there must have been a fertile F_1 present in the beginning. If A and B give a sterile hybrid, how can B be derived from A, unless self-fertilization be assumed? There are two ways of escaping this difficulty: One may suppose the hybrid not to be completely sterile (in which case, as the sterility is decreased, it becomes easier for the condition to become established, but its effectiveness in leading to failure of cross-mating decreases), or one may assume that the sterility arose by two steps. In the latter case, one may assume two pairs of complementary genes, Ss and Tt, such that Ss Tt is sterile, but SS tt, Ss tt, ss TT, ss Tt, and ss tt are

fertile. The derivation of the two cross-sterile homozygous types SS tt and ss TT then involves the production, at some stage, of a type (ss tt) that is fertile with both (see elaboration by Dobzhansky, 1937).

Fisher's analysis is based on the assumption that the two diverging groups have become adapted, through selection, to different conditions (usually geographical), and that the hybrids between them are less well adapted to either set of conditions.

There are, however, certain other systems which will lead to the same type of situation, without any difference in the selective agencies acting on the two populations—without any initial differences between them in gene-frequencies. Perhaps the simplest example is that of reciprocal translocations. Here the heterozygote has a reduced fertility, owing to the more or less frequent occurrence of "irregular" segregation. The smaller the pieces of chromosome exchanged, the greater is the frequency of irregular segregation—but also the greater is the chance that some of the products of such irregular segregation may have some degree of viability and fertility (since they will carry duplications or deficiencies for fewer genes). It is presumably for this reason that small, rather than large, translocations appear to have become established in the phylogeny of *Drosophila* species (Sturtevant and Tan, 1937). This system suffers from the disadvantage referred to above—the greater the sterility of the F_1, the greater the difficulty of establishing the translocation in the first instance. It has been shown by Dobzhansky and Tan (1936) that several small translocations have in fact occurred in the course of the differentiation between *Drosophila pseudoobscura* and *D. miranda;* accordingly this method must be considered as a possible one, though the inversion system now to be discussed seems more likely to be of general occurrence.

Another type of chromosome aberration that should have similar effects is furnished by inversions. If two chromosomes differ by a single inversion within an arm, there exists a mechanism such that there is only a negligible effect of crossing over on the fertility of the heterozygote (Sturtevant and Beadle, 1936). If, however, an inversion is followed by a second one not greatly different from it, the situation is otherwise. For example: suppose a chromosome ABCDEFGH is inverted to form AGFEDCBH, and the latter in turn gives rise to AGCDEFBH—it being assumed that the spindle-attachment is at A or H. A heterozygote carrying the first and the third of the above chromosomes may give crossing over in the CDEF region, and the chromatids resulting from such crossing over (one carrying two B's, no G, the other carrying two G's, no B) will pass to the functional egg cells at random (Gershenson, 1932; Sturtevant and Beadle, 1936). The result is that such a heterozygote has a definitely decreased fertility. In the limiting case the decrease will amount to 50 per cent.

Such a system is not imaginary. The properties outlined have been experimentally determined; and the existence of inversions in wild populations is now known to be frequent and widespread. It may be concluded that they have in fact often led to the production of relatively infertile combinations. Given these facts, there is present all that is needed for the initiation of the selective process outlined by Fisher. It is possible to postulate such an origin for the two races (A and B) of *Drosophila pseudoobscura* that give sterile F_1 hybrids. These two races do in fact show some degree of sexual isolation; and the most frequent sequence in the third chromosomes of race A

(Arrowhead) is so related to the most frequent one of race B (Klamath) that crossovers of the type described should be relatively frequent (Dobzhansky and Sturtevant, 1938).

This case illustrates a difficulty encountered in such an analysis as is here attempted. The male hybrids between the two races are completely sterile, yet the postulated mechanism does not permit complete sterility at the initial stage. In fact, it is clear that in organisms that cannot self-fertilize, this must usually be so, as pointed out above. There thus arises a difficulty which was recognized by Darwin: the most that can be supposed in the early stages of differentiation is that the hybrid is relatively infertile. This infertility must be supposed to increase up to complete sterility—yet at first sight it seems that a decrease in fertility is precisely what selection cannot accomplish.

I have recently discussed (*Essay II* of this series) a case in which sterile individuals must be supposed to have increased in frequency because of selection. The general method invoked in that case may be so modified as to apply here also—namely, we may suppose that the area between the two incipient forms is occupied by a series of semi-independent small sub-populations, only occasionally interbreeding and ultimately competing with each other.

Within a mixed population, the hybrids themselves, when they do breed, will produce up to half their offspring like themselves. Therefore such fertility as they possess will be a source of new semi-sterile individuals. Selection will operate to increase the fertility of this class; but within a small sub-population, the chance establishment of any genes that decrease still further the fertility of the hybrids will increase the efficiency of the sub-population as a whole, and will give it an advantage over the competing sub-populations. Under these conditions, then, it may be supposed that the fertility of the hybrid may be decreased, while the frequency of crossing of the two races is still decreasing.

It is thus possible to formulate a scheme for the origin of interspecific sterility, starting with Fisher's demonstration that races giving a relatively infertile hybrid will tend to accumulate genes that tend to prevent crossing. Appropriate use of Wright's (1932) idea of competing sub-populations makes it possible to see how the infertility can be increased by selection. Inversions and small translocations furnish ever-present materials for initiating or for increasing such infertility of the hybrids—though it is clear that gene mutations must also be concerned, since not all interspecific sterility can be attributed to chromosome rearrangements (see Dobzhansky, 1937).

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

OF HUMAN FOLLY

Being a discussion of *The Symbols of Government* (1935. Pp. vii + 278), to be referred to hereinafter as SG, and of *The Folklore of Capitalism* (1937. Pp. vii + 400) to be referred to as FC; both these books having been written by *Thurman W. Arnold* and published by the *Yale University Press, New Haven, Connecticut.*

By Raymond Pearl, Department of Biology, School of Hygiene, Johns Hopkins University.

I

A primary objective of human biology as a scientific discipline is to find out as much as possible about how human beings behave and why they act as they do. Apparently a considerable number of persons are of the opinion that Mr. Thurman W. Arnold, who is a professor of law in Yale University and at the moment also a higher official of the federal Department of Justice, has made an important contribution towards an understanding of human behavior in these two books. So widely has this view prevailed that pressure has been exerted by the readers of the QUARTERLY REVIEW OF BIOLOGY to have the case examined critically and reported upon in the pages of this family magazine. This we gladly do in the spirit of helpfulness that has ever been our editorial purpose and guide. But beyond this altruistic and humanitarian motivation there is a further, and in our view more important, consideration. It is that in the future it will come to be more clearly recognized than is now the case that in the most complex patterns of human behavior—social, economic, legal, and governmental—underlying biological factors and forces, consequent upon the fact that man is first of all an animal, play a rôle of the very foremost rank of importance. The influence of these purely biological factors and forces will have to be justly appraised before any really penetrating understanding and interpretation of human actions can be reached. The biologist will in time come to take his part and place of equal right in the discussion of matters that have hitherto been regarded as the exclusive territory of sociologists, economists, and lawyers. This idea is in no sense novel—Aristotle was well aware of it, and Thomas Henry Huxley and Francis Galton made important contributions in conformity with it. But now it is rapidly gaining added momentum. This is not solely because it has come to be recognized as a fundamental element in the preaching and practice of modern psychiatrists, and notably Freud. Other forces are at work. During the last fifty years, and particularly in the last twenty-five, the mass alterations of human behavior and attitudes have made it transparently clear that the older modes of thought in sociology, economics, and law are not as helpful as could be wished. These disciplines tend more and more every day

to turn to the biologists for aid in their troubles. So it seems probable that few people will be shocked and most people will think it entirely natural and proper that books dealing in the concrete sphere with matters of law, politics, and government should be discussed in a biological journal.

II

The discussion of these two books before us may well begin with a consideraton of their more general characteristics, after which we can get on to particulars. And let the keynote of the composition be C-majorish—general approbation and appreciation of the cleverness, wit, and humor of the writing. Arnold says (SG, 257): "The keenest objective observations about government in the past has [*sic*] been spoken by satirists and humorists and instantly recognized by the mass of people who laughed with them." In these volumes he has used this technique of exegesis with notable skill, and almost uniformly throughout. Many a chuckle and some belly-laughs are neatly stowed away in the cargo of these books. Only fairly infrequently is the error of laboring the wit to force the chuckle conspicuous.

The theme of the books is the folly and stupidity of mankind, in respect of its behavior about matters in the realms of sociology, economics, law, and government. Making fun of man's foolishness is, of course, a very old story. Of all literary formulas it is probably, on the whole, the surest-fire laugh-getter. Furthermore its power to make for the practitioner a reputation of realizable lifetime asset value at its lower levels of skill, and in perpetuity at the higher levels, is not to be under-estimated. Lucian, Rabelais, and Voltaire, to name but a few out of a longish list, did well for themselves by poking fun at the stupidity and folly of their fellow-men. The reason for the unfailing success of the formula is well understood. It embodies the universal and all-embracing sop to human vanity. Each and every reader of writings embodying the formula identifies himself with the author, and says, in effect: "Isn't it good to be different from these silly, comic jackasses and boobs so justly depicted?" Everyman tells all his friends at the club that the author is a devilish clever chap and that they must get the book and read it at once. His sound confidence that every one of them will react to the formula as he does demonstrates at one and the same time both the truly protoplasmic profoundness of his understanding of human nature in this respect, and the dependably widespread failure of human beings to think of the statistical implications of simple ideas.

It seems certain that many persons think that what these two books say about government and cognate matters is new and important. At least the jacket blurbs on both are calculated to convey this impression. But actually, as Prof. Arnold himself says (SG, 257): "Certainly there are no new ideas about government in this book which were not familiar to Rabelais, or to Swift, or to . . . countless others." If the author is clear on this point his readers should also be, and recognize that the significance of these books, insofar as they are important at all, lies in the fact that they give a fresh description, analysis, and logical demonstration of the stupidity and folly of mankind with respect to another specified frame of reference or universe of discourse.

The particular kind of folly that is abhorred, ridiculed, and objurgated is again an old, well-known, and nearly universal one—the inadvertent confusion or deliberate substitution of abstractions for concrete realities in the conduct of life and affairs. The abstractions are held to take on the qualities of symbols and to lead to elaborate mythologies and folklores that guide and determine attitudes and actions, while phenomena very different from what is envisaged in the mythology and folklore are actually occurring. "When men are confronted with a contradiction between their myths and reality, they have only two recourses. The first is ceremony, drums, and oratory. The second is reason and dialectic." (FC, 115). So SG and FC are devoted to showing in rather pedantic, repetitive detail how idiotically comic and at the same time tragically inhumane and impractical in their results and effects are ceremony, drums, oratory, and reason.

The two books are substantially identical in content. Either one contains no ideas not in the other, though the examples used to illustrate poor humanity's follies are to a considerable extent different in the two books. The second (FC) is constructed after a more popular pattern, chiefly by omitting discussions of more highly technical legal situations that were very interestingly employed as illustrations in the first (SG). The effectiveness of this slight alteration of technique has been reflected in the comparative sales of the two volumes.

One other general characteristic must be mentioned. Any shrewd person who would exploit to its fullest the formula that the follies and stupidities of everybody but himself and his particular friends are funny, realizes that he must be careful to maintain at all points his detached urbanity and amused indifference. Especially the more notable practitioners of this art in the past have been extremely wary about any overt expression of an interest in the improvement of this imperfect world. The earnest uplifter and the brilliant satirist have rarely been amalgamated in one and the same identical corporeal frame. On this point the present treatises fall the merest shade of a thought of a hair's breadth short of the perfection achieved in *Gargantua* and *Pantagruel*, let us say. SG and FC are cagey books, to be sure—a chapter subhead (of Chap. XIII of FC) reads "In which the author plays safe and refuses to be specific"—but nonetheless there indubitably emerges, as the reader goes on, clear evidence of *le pathétique désir d'un monde meilleur*. While there is never anything so crass as the passionate for-God's-sake-let's-do-something attitude, it is made apparent in various subtle ways that if sufficiently urged the author might be willing to sacrifice his preference for lofty critical detachment, and show us how to fix it so that the government, the courts, business, and things in general will not be so funny or so stupid as they have been. In fact his present activities in Washington may not unreasonably be taken as evidence that he has already been seduced by such a siren voice. As Lecky pointed out long ago: "Impelled by a species of moral gravitation, the inquirer will glide insensibly to the system which is congruous to his disposition."

III

Careful reading of the two books indicates clearly enough the broad outlines of the technique by which Prof. Arnold would improve this poor idiotic country of ours. We are started from the position that government as it now is, and particularly as it was before 1933, is bad and wrong because we mistake symbols for reality; are guided by myths; and try to make governmental actions conform to principles instead of to political practicality. Then certain broad ideals for a more perfect kind of government are advanced (and re-advanced), explained (and re-explained), and advocated (and re-advocated). These ideals are fairly embodied in the following statement: Let government be frankly and overtly by *men* with skill for political *organization* rather than by principles and reason, and let it be freed from restrictive legal, social or economic symbols and myths such as are embodied in courts for example, and it will almost if not quite certainly be automatically both *practical* and *humanitarian*. This statement of the essential constructive message of the two books is more bald, specific, and concise than their own more tediously verbose amplification of the matter. But it seems a just summary of the position. The essentials in it are the four words italicized: *men*, *organization*, *practical*, and *humanitarian*. Prof. Arnold apparently sets great store by these words and the ideas they embody. And like most writers of his ilk nowadays he is much interested in semantics generally, especially semantics for other people. Indeed there is a whole chapter (VII of FC) devoted to *The Traps Which Lie in Definitions and Polar Words*. "Justice" and "injustice" are typical polar words.

> The battle between justice and injustice is a . . . struggle. It creates activity. It leads to change. It also leads to civil wars. What we call 'progress' is a consequence of this activity, as well as what we call 'reaction.' Our enthusiasms are aroused by these words and therefore they are excellent tools with which to push people around. Both the Rebels and the Loyalists in Spain are fighting for justice.

That is what enables them to kill so many people in such a consecrated way.

Since justice is a nice word, we refuse to apply it to people who are struggling for things we do not like. The pacifist will refuse to admit that any war can be a war for justice. The born fighter will say that men who refuse to fight for justice do not really care for justice at all. Each side gets morale from the use of such terms and obtains the confidence necessary to make faces at the other side, knowing that God is with him. However, these polar terms are purely inspirational. They are not guides. Each side always claims to have 'justice' on its side. Even organized criminals fight each other in the interest of justice.

All this is very fine and smart, of course. But are *organization*, *practical*, or *humanitarian* any less "polar" or inspirational words than *justice*? One fears not.

The general idea that practical politicians will operate government sensibly and effectively if not too much hampered by legalistic mythology is not precisely a new one. Indeed it has been tried at various times and places. And it has in fact worked moderately well over fair periods of time and again in various places. But, of course, what has hitherto always eventually disturbed the even and placid tenor of its way has been the development of one or another of these unfortunate myths and symbols by the foolish people—myths about the desirability of freedom and liberty; erroneous symbolic notions that practical politicians were not benevolent even though they liked to be called Great Humanitarians, but rather were damned selfish rascals bent only on keeping the power they had; and other similar delusions to which common folk are addicted.

But in an ideal point of view this difficulty could easily be got around, as is shown in SG, 232–236 *passim*.

From a humanitarian point of view the best government is that which we find in an insane asylum. In such a government the physicians in charge do not separate the ideas of the insane into any separate sciences such as law, economics, and sociology; nor then instruct the insane in the intricacies of these three sciences. Nor do they argue with the insane as to the soundness or unsoundness of their ideas. Their aim is to make the inmates of the asylum as comfortable as possible, regardless of their respective moral deserts. In this they are limited only by the facilities of the institution. It is, of course, theoretically possible to treat the various ideas and taboos which affect modern society, just as the alienist treats the delusions of his patients as factors which condition their behavior. This precludes any classification into sound or unsound theories.

The advantages of such a theory for purposes of thinking about government are that we escape the troublesome assumption that the human race is rational. We need not condemn policies which contradict each other solely on the ground that the action of government must be logically consistent. We need not constantly worry about permanent cures, and discard day-to-day policies because of their effects on the morale of the irrational people we are governing in the future. We need not delay such necessary undertakings as public relief because we are worried about their effect on the character of the recipients. We need not compel persons on relief to pauperize themselves and surrender the insurance policies which may afford future relief to their children because of a moral notion that no one is entitled to relief who is not a pauper. The theory eliminates from our thinking the moral ideals which hamper us wherever a governmental institution takes practical action—ideals which create the necessity of a *sub rosa* political machine.

Such a theory allows us to realize that cultures do not change because of the imposition of forms of government, and that the real contribution of new theories can only be to provide a faith which permits men to do practical and humanitarian things in government with confidence that they are not leading to some undefined danger.

The concept of government as an insane asylum liberates us from the notion that wise men think up principles and schemes of government for their duller fellows to learn and follow, and that thus social change is accomplished. It frees us from the notion that "thinking men" decide between the relative merits of communism and capitalism, and choose the better form. Finally, the theory is based on a humanitarian ideal which seems to be indestructible in the march of society—the ideal that it is a good thing to make people comfortable if the means exist by which it can be done.

Beautiful as this scheme is shown to be, it is reluctantly admitted (SG, 236) that "it will never work as a general political theory. Its realism is too apparent, as also is its implied scorn of the human race. The reasons for its failure are the reasons for the failure of all commonsense theories of government in a rational world." After this plaintive, wistful, and pathetic sigh for the *monde meilleur* the author goes on to list and discuss these reasons, but oddly does not include among them that simple one which might naively have been thought importantly obvious, namely that we are not all crazy yet.

IV

Even though the insane asylum idea probably will fail us practically as Prof.

Arnold suggests, all is not lost. A socially constructive thought is offered that may turn the trick (SG, 263, 264).

> The fundamental social axiom of the past was that man, by working only for his personal profit, in the long run produced the most ideal social results. Of course this profit motive had to be checked and balanced against its own excesses by law, by ethics, and by religion. But thus curbed it was part of nature's great plan. Attempts to interfere with it led to disaster. Great institutions like the law and the church, representing contradictory ideals, had to be carefully insulated from control of practical affairs by devices like trial by combat. And thus arose a spiritual government scattered between Washington and the various state capitols, and a temporal government scattered between New York and the various state financial centers, the one representing the great ideals and the other the fundamental axioms of social control.
>
> We suggest that the formula of the new social philosophy which is appearing may be the fundamental axiom that man works only for his fellow man; that it is *this* tendency which must be curbed by law, ethics, and common sense, so that there may be incidental room in the system for the man who works only for personal gain, just as there was incidental room in the old economic creed for the humanitarian. Under the profit creed the chief danger was from well-meaning but impractical humanitarians. Under the new creed the chief danger will be from well-meaning but impractical profit takers.
>
> Sometimes in clear outline, sometimes in strange and distorted and destructive forms, the new conception of man as a creature who does not work for himself is appearing all over the world. In Germany and in Italy the normal man is supposed only to work for his fellow countrymen, and the difference between social organization in peace and war has faded. In Russia the normal man is thought of as one who works for the toiling masses of the world. In all of these countries the axiom that the normal man in the long run works only for his own profit is put down as dangerous radicalism: Fanatical devotion to this single ideal is such that it makes human liberty an unimportant value, and even kindness is stifled for purely humanitarian motives.

This idea of a society in which men work only for their fellow men is extraordinarily interesting to the biologist. For elaborate social organizations of precisely this sort exist here and now in great abundance and variety. Furthermore they have been thoroughly studied by competent biologists. They are found in highest and purest development among the termites, with illuminating variants among the ants and other social insects. It seems odd that Prof. Arnold missed or neglected the opportunity to mention this, and to develop more richly the diverse aspects, implications, and consequences of this social utopia from the experience of the termites. Their social organization has really solved so many of the problems that vex us, like agriculture, unemployment, housing, the Supreme Court, and so on, that it should be helpful to any great humanitarian to study the literature of the subject. Furthermore there is little doubt in the minds of many thoughtful human biologists that the present evolutionary trend of human society is definitely towards the termite plan, or that the rate of progression towards this stable goal is steadily accelerating.

V

The primary purpose of these two books under review is not, however, to advance a theory of government, to plan utopias, or to be constructive generally. It is rather to demonstrate by argument and example that the overwhelmingly important, if not indeed the sole, causes of the present ills of the world are the folly and stupidity about the myths, symbols, and folklore that have determined social, economic, and governmental action. These ills include social unrest, economic depressions, unemployment, war, and various other mass discomforts and disabilities. Prof. Arnold unquestionably makes a strong and convincing case that the myths and symbols are in some part causal elements in the picture. The case is not, however, convincing that they are the sole causes. Nor is it entirely convincing that they merit the first rank of importance in the premises. There are two considerations that need brief discussion here. The first is the general one that while, on the one hand, the present exacerbation of the world's ills is *of the present time*—say of the first third of the twentieth century—and is of a severity and widespread distribution over the face of the earth unparalleled in recorded history; it is equally true that, on the other hand, the folly of mankind in respect of myths and symbols *is not peculiar to the present time*, but on the contrary is not only as old as man himself but has been in continuous and universal operation as long as he has existed. Yet

as a matter of recorded history there have been quite long periods and various places where these myths and symbols did not produce the sort of ills from which we are now suffering. Instead things went along pretty quietly and peacefully, people were comfortable and happy, and the arts and sciences progressed. Such facts cannot be neglected in considering the myth-symbol idea as primary cause of the present world situation.

The second, and more special, consideration is that two factors in the case that seem to the human biologist of the first rank of importance as contributory causes along with myths and symbols, are completely neglected in these books, even to the point of not being mentioned at all. The first of these is the extraordinary growth of world population since roughly the middle of the seventeenth century. In the short period of 300 years or a little less the numbers of human beings living on the face of the earth have multiplied nearly *five-fold*. This has led to an average population density over the whole earth at the present time of approximately 41 persons per square mile of land area. And this means literally *all* the land area—good land, completely worthless land impossible for human habitation and sustenance, and mediocre land. There are solid grounds for the view that this world population density is in itself, directly and indirectly, a potent causal factor behind the world's present unrest and ills. The evidence for this view has been discussed by the writer elsewhere (*The Natural History of Population*, Oxford Univ. Press, 1938) and cannot be further elaborated here. The only point here is that it cannot wisely be left out of the picture Prof. Arnold essays to paint.

The other factor that is neglected is basically a political one. It is that generally, and with some admitted but relatively unimportant local exceptions, the trend of social, political, and economic evolution since the early part of the nineteenth century has been towards ever greater and greater *concentration of power* in the hands of very small groups relative to the total number of human beings struggling to survive on the earth. Of prime significance is the trend towards increased concentration of governmental power in central at the expense of local agencies. Economic power has similarly become more and more concentrated with the development of trusts, monopolies, great industrial organizations, and so on. All of this Prof. Arnold discusses. Some of it has his approval, especially central concentration of governmental power because there can thus be "practical common-sense" government. But the point that the human biologist would raise is that all and any concentration of power tends to cause human unrest and discomfort, because long and painful experience has demonstrated that no human being can be trusted with much power of any sort very long. If any axiom can be said to be demonstrated by experience it is that the more concentrated the power of a government is the more intolerable that government becomes as time passes. Every benevolent despot (and of course all despots are benevolent if you let them tell it) has shared Prof. Arnold's view that men are boobs and fools, only to have it revealed in the sad end that they can also be fighting fools.

VI

The evolution of law and government has been, in technical biological lingo, a continuing and progressive attempt at group adaptation to meet environmental difficulties consequent upon sociality. When numbers of men had to, or chose to, live together in a spatially limited territory, rules of conduct were devised and by common, if never quite universal, consent agreed to, in order to ensure that the group should be protected so far as possible from annoyance, abuse, and damage by individuals determined for whatever reason to be nuisances. This process has crystallized out laws and government, the latter being the mechanism to ensure the observance of the rules as effectively as might be. But sensible men have always been of the opinion that the least government was the best government. Now any historically informed person knows that this process, in common with all truly evolutionary processes of which we have adequate knowledge,

has been slow, inefficient, and characterized by an enormous amount of foolish and bungling trial and error. But it has steadily gone on, in fact. As the number of human beings living together increased, and the techniques of getting a living multiplied, the problems of rule-making and administration of the rules naturally become more and more complex and difficult to solve, and the imperfections of the attempts more apparent and troublesome. It is not difficult to pick out and emphasize, as Prof. Arnold has done, the conspicuously bad and foolish elements that have accrued as this long and muddled process has gone on. But they do not quite make the complete picture. Men are foolish fellows, of course, and have made a lot of mistakes. But they have corrected a lot of mistakes too. And if man can manage to continue to survive as a species, about which there is always some doubt, a lot more mistakes will very slowly but surely be corrected. For such is the normal way of evolution.

What is often forgotten or overlooked is the terrible slowness of organic evolution. Man's most recent acquisition evolutionally is that part of his brain that is associated with conscious thought and the capacity for reasoning. In the long eons of time that have been required in the evolution of living things from the simplest protists to man it was just a little while ago that forebrains began to bulge out to make highbrows. Is it any wonder that men are still foolish in the head? But let it not be forgotten that in the enormously longer time that it had to do it in man's body has achieved a wisdom that is superb in its precision, accuracy, and efficiency. It is reasonably to be expected on the basis of past experience, again with the proviso that he manages to survive as a species, that in the passage of many, many thousands of years the wisdom of man's mind may become more nearly equivalent to the wisdom of his body than it now is. The patient, the meek, and the humble—the "foolish," in short—will probably manage somehow to struggle along and to view this prospect with some degree of equanimity because they will sense that there is nothing very immediate or startling that they can do about it. Bright and witty messiahs, whom we shall long have with us, will continue to be irked because the process is so slow, so inefficient, and neither "practical," "political" nor "humanitarian."

BRIEF NOTICES

EVOLUTION

A HISTORY OF LAND MAMMALS OF THE WESTERN HEMISPHERE. *Revised Edition Rewritten Throughout.*

By William B. Scott. Illustrated by R. Bruce Horsfall and Charles R. Knight. The Macmillan Co., New York. $7.50. 8½ x 5½; xiv + 768; 1937.

It was just a little more than 60 years ago that the author of this volume together with the late Henry F. Osborn and Francis Speir, Jr. planned and carried out their first palaeontological expedition into the Bad Lands of southwestern Wyoming. They entered an almost virgin field but one abundantly rich in material. In both the western part of North America and in southernmost South America there had been preserved marvelous series of records of the successive assemblages of animals that once dwelt in these regions. The present book (first appearing in 1913 but now entirely rewritten, except for the chapter on the skeleton and teeth) brings together all of the important links in the ancient history of mammals as told by fossilized forms and so far deciphered by the painstaking efforts of various investigators, of whom Professor Scott is one of the foremost leaders.

The book, which will long be a classic, should be in all biological libraries for it will be invaluable to teachers as well as students in this field. It will also be of great interest to the general reader. Fossilized animals were never given common names, therefore Professor Scott has used throughout the technical nomenclature, but this can hardly be considered a hindrance to the enjoyment of the book since the numerous remarkably fine illustrations (420 line drawings and photographic plates—those that are new being the con-

tributions of R. Bruce Horsfall) aid materially in comprehending the text. A detailed index is included.

THE NATURE AND MEANING OF EVIL AND SUFFERING AS SEEN FROM THE EVOLUTIONARY STANDPOINT.

By C. J. Bond. H. K. Lewis and Co., London. 1s. 8⅝ x 5½; 29; 1937 (paper).

An interesting discussion of two theses: (1) that evil is a relative term, and is caused by strong individualism, and (2) that suffering is a result of our ethical evolution not being able to keep pace with cosmic evolution. The conclusion is that evil and suffering in the world will become extinct only when we have a common universal race, a common religion, and a common system of education. The text is developed in a logical manner, and presents much evidence in support of the basic ideas. However, there have been very few books written which can boast, as this one can, that more than 75 per cent of its paragraphs contain only one sentence.

ENTSTEHUNG DER MENSCHENRASSEN.

By Hans Weinert. Ferdinand Enke Verlag, Stuttgart. RM. 17. (paper); RM. 18.80 (cloth). (Outside of Germany, 25 per cent less). 10 x 6¼; viii + 313; 1938.

This little book concerning the origin of human races contains little that is actually new on the subject, but it is an excellent summary of all that is known about prehistoric man from the standpoint of physical anthropology. Five chapters are devoted to Neanderthal and pre-Neanderthal races, one chapter each to Diluvian man, Mesolithic man, Neolithic man, and present day races, with special emphasis on present day European white races. There is a bibliography and index.

GENETICS

HEREDITY AND POLITICS.

By J. B. S. Haldane. W. W. Norton and Co., New York. $2.50. 8 x 5½; 202; 1938.

In this book, based on a series of Muirhead Lectures at the University of Birmingham, Professor Haldane gives a popular exposition of our present knowledge of human genetics and of the bearing on such measures as compulsory sterilization and anti-Semitic legislation. When an abnormal condition is due to a dominant gene that manifests itself in 100 per cent of cases and early in life, sterilization would cut off all hereditary cases of the defect, leaving only those cases due to mutation. Where the condition is recessive sterilization would not produce a noticeable effect in less than thirty or forty generations, whereas the prohibition of cousin marriages would reduce the incidence of the abnormality appreciably.

Although it is often stated that the children of two mental defectives are all defective, it was found that in Birmingham of thirteen children both of whose parents were defective "only one was pronounced defective in 1933 and another one may possibly be registered by now." Of 345 children at least one of whose parents was defective only 7.5 per cent were defective, 18.5 per cent were backward, 71 per cent were normal and 3 per cent were above the average. Professor Haldane concluded "that the sterilization of all mental defectives would probably cut down the supply of mental defectives in the next generation by something of the order of 10 per cent." If such a reduction is thought sufficient to warrant measures to prevent mental defectives from having children, he favors segregation in institutions as more humane than sterilization.

PHYSIOLOGICAL GENETICS.

By Richard Goldschmidt. McGraw-Hill Book Co., New York. $4.00. 9 x 6⅛; ix + 375; 1938.

With the ever growing interest in the field of genetics, and with the increasing amount of experimental work and the development of new methods in the study of heredity has come the need for someone to synthesize the fundamental findings of the different workers into a comprehensive volume. Just such a synthesis Goldschmidt has attempted in this volume. He not only has had long experience in experimental genetics, but he has the rare

ability to combine his own work with the work of others into a very interesting and readable book. The author is entirely justified in stating in his preface that "Not only has this material been assembled and reviewed, but an attempt has been made to organize it into the skeleton of a future science of physiological genetics." The text is quite technical throughout, but it is assumed that the reader has enough background in genetics and embryology to understand the fundamental principles concerned even if he does not understand all the technicalities. The 44 page bibliography, the author index, and the subject index add to the usefulness of the volume. All geneticists will not agree with some of the theoretical positions taken.

FAKTORENKOPPELUNG UND FAKTORENAUSTAUSCH BEI NORMALEM UND ABERRANTEM CHROMOSOMENBESTAND. *Probleme der theoretischen und angewandten Genetik und deren Grenzgebiete.*

By Wilhelm Ludwig. Georg Thieme Verlag, Leipzig. RM. 11. 8¼ x 5¾; 245; 1938 (paper).

The problem of crossing-over and exchange of parts between two or more chromosomes is one of the most important and complicated chapters in modern genetics. The author gives a very concise insight into these problems—the genetic theory as well as the cytological fundamentals. The presentation is hardly suitable for beginners in biology and genetics, but the well-grounded student will profit from this special review which covers almost all of the newer investigations. As mathematical symbols play an important rôle in genetic thinking these can not necessarily be avoided in some of the discussions. There is an exhaustive list of the literature up to 1937, and indices of authors and subjects.

SPONTANE UND STRAHLENINDUZIERTE MUTABILITÄT.

By H. Stubbe. Georg Thieme Verlag, Leipzig. RM. 6.80. (Outside of Germany, except Palestine, 25 per cent less). 8¼ x 5¾; 190; 1937 (paper).

This little book is intended as a review and summary of the most recent work done in the field of mutations. After an introduction to the mutation theory, the author discusses at some length different types of spontaneous mutations, their effects, their manner of origin, dependence on temperature, etc. By far the greater portion of the book however is devoted to X-ray mutations. There is a lengthy discussion of H. J. Muller's contributions on the methods, results, and generalizations that can be drawn from X-ray genetics, the production of chromosome and gene mutations from short wave rays in animals and plants, and practical results particularly for the field of botany. There is a 20 page bibliography and an index.

FORTSCHRITTE DER ERBPATHOLOGIE, RASSENHYGIENE UND IHRER GRENZGEBIETE. *I. Jahrg., Heft 1 and 2.*

Edited by Johannes Schottky and Frhr. von Verschuer. Georg Thieme Verlag, Leipzig. Subscription price RM. 16. plus postage. 10 x 6¾; 134; 1937 (paper).

This new journal is concerned mainly with pathological genetics connected with race hygiene. A few of the articles in the first two numbers of Volume I have the following titles: Inheritance of intelligence and character, by K. Gottschaldt; Selection, by H. Wülker; Inventory of inheritance, by H. Schade; Discoveries in the inheritance of schizophrenia and cyclothymia, by H. Luxenburger; Epilepsy, by H. Geyer; and Inheritance of dental malformations, by F. Proell.

The journal has so far managed to accept articles of real scientific value and free from any political propaganda. This is unusual for a publication from Germany dealing with race hygiene and it is to be hoped the editors will continue to select articles of such calibre.

GENERAL BIOLOGY

THE AXIOMATIC METHOD IN BIOLOGY.

By J. H. Woodger. With Appendices by Alfred Tarski and W. F. Floyd. University Press, Cambridge; The Macmillan

Co., New York. $3.75. 8½ x 5½; x + 174; 1937.

In 1930 and 1931, J. H. Woodger published in Q. R. B. (Vol. 5 and 6) a noteworthy essay in three parts entitled *The "Concept of Organism" and the Relation between Embryology and Genetics* in which modern symbolic logic was applied to biology. This excellent paper has now been followed by a book in which the first steps towards treating genetics, embryology, and taxonomy from an axiomatic point of view have been taken. The result is a work of signal importance in so far as it not only inaugurates a new subdivision of biology but also applies to biology a philosophical instrument which raises the philosophy of biology, and indirectly the philosophy of science, to a new position of eminence. Woodger has taken a long stride forward towards inculcating methods of exactness and rigor into biology, and representing the philosophy of science as the discipline of exact definitions, explicitly stated assumptions, and the rigorous deduction of logically related theorems. Thus as a biological work, *The Axiomatic Method in Biology* stands as the inceptor of a new biological science, while as a philosophical work it deserves a place of prominence as a guide and example to future endeavors in philosophical research among scientific topics.

The first two chapters contain a brief summary of those logical concepts and methods which are essential to an understanding of their subsequent application to biology. Though these are admirably presented, they are insufficient in themselves to prepare a reader, unfamiliar with symbolic logic, for the chief topic of this book. It should be recommended to such a reader that he first acquaint himself with Carnap's *Abriss der Logistik*, the influence of which is clearly displayed throughout Woodger's work.

THE HUMAN VALUE OF BIOLOGY.

By Johan Hjort. Harvard University Press, Cambridge. $2.50. 7½ x 5⅛; xii + 241; 1938.

This absorbing little volume is a revision of a series of lectures given by the author before his students at the University of Oslo, and presents in a dramatic fashion a summary of his philosophy of life. Hjort's study of marine populations has led him to believe that much knowledge can be gained from a study of populations of lower forms which can, either directly or indirectly, be applied to human populations in our effort to bring about a better social order.

Historical evidence is presented to show that the different philosophies have contributed to the development of biology, but it makes the point that present day biology, to be of the highest human value, must be interpreted by biologists, and not by philosophers of pure reason, mathematics, or physics. Hjort summarizes his views in the idea that an ideal goal for any science, in fact, for any unit of society, is to aid in . . . "liberating those who suffer from want of intellectual activity in their work, the greatest of all social diseases." There is a bibliography.

LABORATORY MANUAL FOR GENERAL BIOLOGY.

By Leslie A. Kenoyer and Henry N. Goddard. Harper and Bros., New York and London. 75 cents. 10¾ x 8½; 73 + 27 plates; 1937 (paper).

LABORATORY DIRECTIONS IN COLLEGE ZOOLOGY. *Revised Edition.*

By Henry L. Bruner. The Macmillan Company, New York. $1.75. 8½ x 5¾; xiv + 163; 1938.

LABORATORY OUTLINES FOR ANIMAL BIOLOGY. *Revised Edition.*

By Michael F. Guyer and Halcyon W. Hallbaum. Harper and Brothers, New York. $1.75. 8½ x 7; xvi + 273; 1937.

The first of these manuals (dealing with both plant and animal forms) is so arranged that by the removal of the cover it can be slipped into a loose-leaf notebook. A group of 27 plates showing line drawings, largely adapted from well-known texts, is provided for the student to label after his work with laboratory specimens is completed. The second manual emphasizes the morphology of animal forms. It fits in particularly well with Hegner's *College Zoology* (fourth edition), and offers adequate material for a full year's work. The last manual is more detailed than the other two. It is

intended to be used in conjunction with the senior author's textbook of *Animal Biology* and also provides a full year's work.

NEW INTRODUCTION TO BIOLOGY. (*Revised*).

By Alfred C. Kinsey. J. B. Lippincott Co., Chicago. $1.76. 7⅞ x 5¼; xv + 845; 1938.

The present edition of this fine high school text follows very closely the content and organization of the earlier edition (Q. R. B., Vol. 9, No. 1). The body of excellent illustrative material, which has had minor omissions, alterations, and additions, is now in such form as to give the content of the text more unity and emphasis with regard to the relationship of structure and function, and to the relationship of organisms to their environment and to each other. The volume contains, as did the earlier edition, a list of select references, a fine glossary, and a complete index.

SOURCE BOOK OF BIOLOGICAL TERMS.

By Axel L. Melander. Department of Biology, The College of the City of New York, New York. $1.10. 8½ x 5⅜; vi + 157; 1937.

The layman will find this book informative, though esoteric; the classical scholar will find that brother scientist has steeped his nomenclature in mythological lore; the biologist will polish up his technical vocabulary along with his Greek and Latin; and the science student will be able to acquire standardized pronunciations and meanings with a technical vocabulary. Part I is a discourse on such problems as word sources, homologies, phylogeny, origins, emergence, derivations both mistaken and uncertain, and mispronunciations. Part II is an alphabetical list of the components of the biological vocabulary. A scholarly book.

BIOLOGY. *The Story of Living Things.*

By George W. Hunter, Herbert E. Walter and George W. Hunter, III. American Book Co., New York. $3.75. 8¾ x 5⅞; x + 670; 1937.

The writers of this excellent elementary text have hit upon something new and appealing in their method of presentation. Every chapter has zest and vitality, and even the usually boring sections on classification have been handled with such precision and finesse that they captivate one's attention and consideration. The text is beautifully illustrated throughout, either diagrammatically or photographically, and each chapter is concluded with a brief bibliography on the subject presented.

LADY JULIA PERCY ISLAND, 1935 EXPEDITION. *Reports of the McCoy Society for Field Investigation and Research. No. 1.*

Proceedings of the Royal Society of Victoria, Vol. 49: 329–437, 1937.

Lady Julia Percy Island, lying off the south coast of Australia and appearing to represent a small and self-contained sample of the Australian environment, was selected as the site of the first season's investigations of the McCoy Society. The island proved to be unique, however, rather than typical when it was found to be purely volcanic instead of a portion of the old continental mass cut off from the mainland. This first report of the Society puts on record all findings made during the summer of 1935–1936 on the state of the balance of nature on this island, and includes lists of the plant and animal forms found.

INTERNATIONAL ASPECTS OF OCEANOGRAPHY. *Oceanographic Data and Provisions for Oceanographic Research.*

By Thomas W. Vaughan and Others. National Academy of Sciences, Washington. Free, (not available for general distribution). 12 x 9; xvii + 225 + 36 plates; 1937.

The purpose of this volume is to index the available literature on geophysical contributions to oceanography and to catalogue the institutions (and their resources) which are adding to the knowledge of the subject. Maps and tables

show the regions in which work has been done on serial sections of temperature and salinity in the different ocean basins, charting the bottom of the oceans, submarine earthquake epicenters, and results of maritime gravity research (1923–1932). The catalogue of institutions engaged in oceanographic work occupies about two-thirds of the book.

THE PENDULUM SWINGS BACK.
By Marvin M. Black. Cokesbury Press, Nashville, Tenn. $2.00. 7⅞ x 5⅛; 229; 1938.

The author of this verbose bilge is of the opinion that "science seems to have gone as far as it can with experimentation," and suggests that biology should now look for its further progress in such directions as telepathy and clairvoyance, the philosophical implications of Lamarckism, and the "soul in medicine."

HUMAN BIOLOGY

GEORGES DREYER. *A Memoir by His Wife. By Margrete Dreyer. With an Introduction by Sir Charles Sherrington. Basil Blackwell, Oxford.* 10s. 6d. net. 8½ x 5½; ix + 249 + 4 plates; 1937.

This book is a worthy memorial of a distinguished immunologist and physiologist. Georges Dreyer was a Dane by parentage and was educated at the University of Copenhagen, where he studied bacteriology and pathology under Salomonsen, and collaborated with Madsen in studies on diphtheria toxin and with Finsen on the biological effects of light. In 1908, at the age of thirty-four, he was elected to the Chair of Pathology at Oxford. During the war he developed an automatic apparatus for supplying the proper concentration of oxygen to flyers at high altitudes and supervised its manufacture for the British and American air forces. This work on the physiology of respiration led him to the study of "Vital Capacity"—the maximum amount of air one is able to expel from his lungs—and its relation to physical fitness.

Much of Dreyer's research was given to the development of more exact quantitative methods in biology and especially in immunology. Thus in his Copenhagen days he developed a standard technique of the Widal reaction with a sensitivity of 99.1 per cent and a specificity of 98.8 per cent. At Oxford he collaborated in research on a diagnostic test for syphilis that was one of the forerunners of the Kahn test. In his Harvey Lecture at Harvard he again championed the cause of biological standards.

> Now, the first question that comes to the mind is: What advantage do we derive from the use of definite standard units? and the answer is readily given: that science-workers of every country and every race in this way are enabled to understand each other for the simple reason that they speak the same language.
>
> This has already been achieved in the field of electricity by the universal acceptance of the well-known standard units, the ampère, the volt, the watt, the ohm, etc., while the civilized world is still divided on such important questions as the system of weight, measure and coinage.
>
> If the goal of a really close co-operation between medical research workers could be reached, the biological sciences would be put on an equal footing with those hitherto accepted as the "exact."
>
> The difficulty of fixing biological standards is only too often believed to be insurmountable because of the idea so widely taught and accepted that any great degree of accuracy in biological science is beyond our reach.
>
> That this difficulty, however, is more imaginary than real should not be doubted when one remembers that some of the fine tests in the "exact" science depend on biological functions, namely, those of sight and hearing. . . .
>
> Of course individuality and independence is admittedly the proper spirit in science and any measure that might tend to interfere with that freedom of thought and action is justly condemned. And as acceptance and use of biological standards often impose the adoption of a definite technique one easily understands the hesitation.
>
> Yet, the use of valid standard units can never really interfere with individual freedom. Hardly anyone, even the greatest of champions for individuality, could maintain that the universal acceptance of units like the ampère, the volt and the ohm has in any possible way interfered with the individuality and originality of modern investigators.
>
> On the contrary, research carried out without the use of these standard units would be lost to the scientific world at large because of the lack of information concerning the values involved. . . .

As will be seen, Dreyer accomplished important work in several fields of medical research. Yet the book deals not only with the scientist but with the man, his friendships, his zest for life. Here was a

man whom it was an inspiration and a delight to count as one's friend.

SEXUALITY IN THE SECOND DECADE. *Monographs of the Society for Research in Child Development, Volume II, Number 3. Serial No. 10.*

By Raymond R. Willoughby. Society for Research in Child Development, National Research Council, Washington. 75 cents. 9⅛ x 6⅛; iv + 57; 1937 (paper).

SEXUAL MATURATION AND THE PHYSICAL GROWTH OF GIRLS AGE SIX TO NINETEEN. *Monographs of the Society for Research in Child Development, Volume II, Number 5. Serial No. 12.*

By Frank K. Shuttleworth. Society for Research in Child Development, National Research Council, Washington. $1.25. 9⅛ x 6⅛; xx + 253; 1937 (paper).

DATA ON THE GROWTH OF PUBLIC SCHOOL CHILDREN (*From the Materials of the Harvard Growth Study*). *Monographs of the Society for Research in Child Development, Volume III, Number 1. Serial No. 14.*

By Walter F. Dearborn, John W. M. Rothney and Frank K. Shuttleworth. Society for Research in Child Development, National Research Council, Washington. $1.00. 9⅛ x 6⅛; 136; 1938 (paper).

The first monograph is a review of those factual studies which, in the opinion of the author, "enable some generalization about adolescent sexuality." Under the headings of *Implicit Sexuality*, *Autosexuality*, *Para- and homosexuality*, and *Heterosexuality* the author presents data from numerous investigations regarding the onset of various types of sexual experiences, very seldom, however, giving any indication as to how the original studies were carried out and what reliability can be attached to the figures so copiously cited. There is a bibliography but no index.

The second monograph is concerned primarily with the statistical techniques applicable in an analysis of longitudinal data. Endocrine factors (time of first menstruation) and their relation to growth patterns were also studied. Altogether 3650 girls from Massachusetts schools were followed, with annual measurements as long as they remained in school. The measurements, in addition to information on the onset of first menstruation, included eleven physical dimensions, dental examinations, intelligence tests, school progress, etc. It was found that "interrelations of assumed endocrine stimulation, advent of menarche, and the timing of patterns of growth are very high." A large number of graphs and tables have been added to the text.

The third monograph presents in detail the repeated growth records acquired over a period of 12 years, on 1553 individuals, these cases being selected for completeness of data from the records of the Harvard Growth Study. The first third of the text is made up of explanatory notes on the tables that follow.

BIO-POLITICS. *An Essay in the Physiology, Pathology and Politics of the Social and Somatic Organism.*

By Morley Roberts. J. M. Dent and Sons, London. 15s. net. 9¼ x 6⅛; xv + 240; 1938.

This remarkable book deserves much more extensive and thorough discussion than limitations of space permit here. Under these circumstances we conceive it to be our first duty to recommend strongly and unreservedly to our readers that they get the book and both read and study it. It is a penetrating and realistic contribution of first rate importance to sociological theory.

Morley Roberts is known to most people only as a novelist. Actually he has been for the greater part of a long life a serious and profound student of biology, and an occasional writer in that field. In this book he develops to something like logical completeness the idea of human society as organism that Herbert Spencer dropped when he perceived that its implications were going to run painfully contrary to some of his most cherished prejudices. A strong case is made for the view that nations display the characteristics of loosely-knit low-grade organisms struggling to survive. It is further made clear that there is no evidence that intellect has played any significant part in the

growth, disorder, or decay of society—in social evolution, in short. Government is more soundly to be apprehended as a set of physiological responses to the internal and external environment in accordance with physical law than as an historically elaborated and intellectually sophisticated instrument.

The book is written with the clarity and charm expected from its author's high literary craftsmanship. It is adequately indexed and carries a soundly selected bibliography. Again we emphasize that it deserves the widest reading, and will repay the reader.

VÖLKER AM ABGRUND. *Zweite vermehrte Auflage.*

By Friedrich Burgdörfer. J. F. Lehmanns Verlag, Munich and Berlin. 3 marks; (outside of Germany) 2.25 marks. $9\frac{7}{8}$ x $6\frac{5}{8}$; 76; 1937 (paper).

Burgdörfer adds another book to his already numerous publications on the dangers of the declining birth-rate for Western civilization. It is not overpopulation, he points out, but underpopulation that now menaces the highly civilized countries of Europe and European stock. The earth could feed 8–10 billions of human beings (instead of around 2 billions as the present population of the earth) and the United States is easily in a position to support 500 to 600 millions of men. But in almost all nations of Western civilization the birth-rate is rapidly decreasing. This is demonstrated by a series of tables and figures on population movement in recent years. The German government alone, since 1933, has checked this danger. After an almost uninterrupted decrease in births, the number of live births is increasing again, amounting in 1933 to 971,000, in 1934 to 1,198,000, in 1935 to 1,264,000 and in 1936 to 1,279,000. That means a birth-rate increasing from 14.7 per 1,000 in 1933 to 19.0 in 1936. However, that is no greater than the birth-rate was in 1928. At the turn of the century it was almost double in Germany, the absolute number being over 2 millions of live births a year. Burgdörfer explains the single methods of the government's economic and social policies in relief of marriages (*Ehestandsdarlehen*), but he furthermore wants to show that by far the more important was the will of reproduction among all groups of the population. So he demonstrates that not only the number of first-born (of the young marriages) have increased but also the second, third, and fourth-born (of the older couples). This publication, of course, has more the character of propaganda (edited as No. 1 in a series on political biology) than of scientific fundamentals. It is illustrated by tables and is indexed.

POPULATION PRESSURE AND ECONOMIC LIFE IN JAPAN.

By Ryoichi Ishii. University of Chicago Press, Chicago. $3.00. $8\frac{1}{2}$ x $5\frac{5}{8}$; xix + 259; 1937.

Although this detailed study of the Japanese demographic and economic situation was completed before the outbreak of the present Sino-Japanese hostilities, it will probably be read in the light of that conflict. After remaining nearly stationary during the eighteenth and early nineteenth centuries the population of Japan has nearly doubled since 1872. As only about 15 per cent of the area of Japan is arable this rapid increase in population has resulted in a density of 27 persons per acre of tilled land as compared with 21 in the Netherlands.

Although the trend of the birth rate has been downward since about 1920 this is counterbalanced by a corresponding decline in the death rate. Although the former hostile attitude of the Japanese government towards birth control is changing, it is doubtful how soon this factor is likely to have an appreciable effect on population growth. According to various estimates the Japanese will have to feed from 15,000,000 to 20,000,000 additional mouths within the next generation. How can they do this? It is estimated that 5,000,000 acres can be added to the agricultural land of Japan. This, together with the proposed increase of acreage in the Japanese colonies and the probable increase in production per acre, will make Japan self-sufficient with

regard to her food supply. However, the cost of food production is relatively high. The best hope of taking care of the increase of Japanese population without reducing the standard of living seems to be a further industrialization of the nation. But an industrial nation poor in natural resources requires access to sources of raw materials as well as a market for its manufactures. The motives for the present conflict in China are therefore evident, but whether the Japanese adventure will solve her difficulties or add to them is not so plain.

AFRICA'S GOD. VII. EAST AFRICA. *Anthropological Series of the Boston College Graduate School, Volume II, Number 4.*

By Joseph J. Williams, S.J. Boston College Press, Chestnut Hill, Mass. $1.00. 9¾ x 6½; 54; 1937 (paper).

An interest in the Negro race, especially in the Jamaican Negroes and their beliefs, has led Dr. Williams to study the parent-stock in Africa. Since accurate observations of the African tribes by one individual is an impossible task, a questionnaire was broadcast "among those who were actually toiling along the required lines throughout the length and breadth of the Dark Continent." The answers, which are still being sent in by persons best fitted to understand each tribe (that is, by missionaries and government officials who have lived many years among the Negroes) are available at the Dinand Library, at Holy Cross College, Worcester, Mass.

The present monograph includes the answers of reliable workers concerning the tribes of Kenya and Tanganyika Territory in East Africa. The conclusions reached are that the tribes of the regions studied are monotheistic in their religious beliefs. The Wafipa of Tanganyika show positive signs of religious decadence, but, "vestiges of monotheism are found among the ancient beliefs of this tribe"; the monotheistic tendency, in general, "looks to a strong Hebraic infiltration that made itself felt all along the territory covered by the present paper."

There is also a brief historical discussion on the Hebrew exodus from Egypt and the Jewish influence on Africa thereafter, as a result of Jewish immigration and colonization.

Dr. Williams's paper is followed by an argument in favor of "Revelation and Primitive Sacrifices" by William J. McGarry, *S.J.*, President of Boston College.

CHINA AT WORK. *An Illustrated Record of the Primitive Industries of China's Masses, Whose Life is Toil, and Thus an Account of Chinese Civilization.*

By Rudolf P. Hommel. Published for the Bucks County Historical Society of Doylestown, Pa. by the John Day Co., New York. $5.00. 10¾ x 8⅛; x + 366; 1937.

This book is the result of an expedition to Central China financed and directed by Henry Chapman Mercer, that versatile historian and antiquarian of Bucks County, Pennsylvania. Its object was to investigate, photograph and describe the implements and methods of labor used in the rural districts, in order to throw further light upon the daily life, character and civilization of the Chinese people in the area studied. No generalizations as to tools or customs are possible in speaking of China.

The photographs and measurements of the implements, which fully illustrate the text, were difficult to take because of the prevalence of superstition and the extreme reluctance of the farmers to let their possessions be photographed. It is hard to realize that these heavy, crude, ancient looking tools are the present-day working equipment of millions of Chinese. There have been few changes in thousands of years. In every photograph and every detailed description of the laborious methods employed there is amazing evidence of the tremendous—almost superhuman—work involved, and the prodigal expenditure of this physical energy. There could be no more graphic way of depicting the traits of infinite patience and long-suffering of the Chinese people than in a study of this kind.

The contents of the book are divided into classifications of tools required for the various phases of life. There is a complete index of six pages.

WE AMERICANS. *A Study of Cleavage in an American City.*

By Elin L. Anderson. Harvard University Press, Cambridge. $3.00. 8 x 5½; xii + 286 + 6 plates; 1937.

Miss Anderson has chosen Burlington, Vermont, for her sociological study of the complex ethnic relationships behind the outward comformities of dress and appearance as seen on active, bustling streets. Burlington is a conservative city with a population of nearly 25,000, in which number are such diverse racial groups as the English, English-Canadians, French-Canadians, Irish, Jews, Italians, Germans, Greeks, and Syrians. This city faces, as do other communities of America, the complex and as yet unsolved problem of ethnic cleavage.

Ethnic distinctions are of prime importance in Burlington, and playing no small part in this segregation is the attitude of the fourth generation Americans, and those who claim even longer ancestral lineage in this country, to the first, second, and even third generation Americans. Each of Burlington's racial groups clings to its own tradition; each has the ethnocentric outlook of the primitive savage, and each is suspicious of the other racial groups. The incorporation of the many racial groups into a unified community of common aims still lies in the future. The story of Burlington indicates how far from realization is "the high dream of creating an American people of all the elements that have been borne into the country on successive waves of immigration."

This study, which has received the third John Anisfield award, profits by its author's sympathetic insight and honest revelation of the racial, religious, social, and economic elements that keep Americans divided into separate ethnic groups.

HARLOW BROOKS *Man and Doctor. Second Edition.*

By John J. Moorhead. Harper and Bros., New York and London. $3.50. 8½ x 5¾; vii + 302 + 9 plates; 1937.

From 1895 until his death, in the forty years in which Harlow Brooks practised his profession in New York City, he not only achieved an important position in the art and science of medicine, but because of genuinely human qualities acquired a host of friends and admirers. The fact that his biography has gone to two editions is a tribute to Brooks's personality and medical reputation as well as to the author's literary skill.

Brooks went directly to New York City after having received his doctor's degree at Michigan and almost immediately he became associated with the Bellevue Hospital Medical College. The author introduces this period of Brooks's life with interesting and often amusing accounts regarding Bellevue Hospital and the leading physicians of the day. From the very beginning, Brooks's uncommon abilities were apparent and he early received due recognition as a pathologist and diagnostician. The high points of his remarkable career including his war services which were almost balked at the start by bureaucratic red-tape are described in a style which is discursive yet not lacking in elegance. The reader is impressed by Brooks's modesty and continuous devotion to the high principles of the art of healing but most of all by the strong affection which he inspired in the author as well as in many others.

SHORT YEARS. *The Life and Letters of John Bruce MacCallum, M.D. 1876–1906.*

By Archibald Malloch. Normandie House, Chicago. $3.50. 9¼ x 6½; xiii + 343; 1938.

John Bruce MacCallum died two months before reaching his thirtieth birthday. But in that short life he achieved a reputation as an invetigator that has sometimes been compared with Frank Balfour's in respect of brilliancy and promise. Outwardly his short life was a simple and uneventful one—boyhood and undergraduate days in Canada, Johns Hopkins Medical School, a bad summer in Germany, and finally the University of California with Jacques Loeb—but over and through it all the never-ceasing battle against tuberculosis.

Dr. Malloch has done a superb job in the labor of love that made this book. Mac-

Callum's own letters tell the story, but the editing, arranging, and running comment have been managed with consummate artistry by Malloch.

This is a book that every medical student —indeed every student of any branch of science—should read. It will reveal the inherent dignity and beauty of scientific research, and set a spiritual model for living that cannot fail of meaning and helpfulness in a disordered world.

SEASON OF BIRTH. *Its Relation to Human Abilities.*

By Ellsworth Huntington. John Wiley and Sons, New York. $3.50. 8½ x 5½; vii + 473; 1938.

Huntington has studied the seasonal fluctuations in the number of births in this and other countries and concludes that: (1) the kind of weather present at about the time of conception influences the mental and physical condition of the embryo and consequently affects the whole life of the individual; (2) the human race inherits a mechanism sensitive to weather changes; (3) the seasonal oscillations in births are due to the survival of individuals whose seasonal reproductive rhythm coincides with the optimum time for the offspring to be born and live; (4) children conceived and born during the optimum season are more vigorous, stronger, etc., than those born at other periods. It is difficult to judge whether any or all of these conclusions are actually supported by the facts. The author presents his data in graphic form only and consequently it is practically impossible to determine whether the relations observed by him are real or simply apparent. Moreover, there are numerous factors which could be casually responsible for the seasonal fluctuations of births and that the author passes over lightly or fails entirely to take into account. Finally the comparability of the data used is open to question and Huntington's interpretation of differences between countries is often very dubious. Better analytic technique and method is needed before the conclusions advanced by the author can be given serious consideration.

RACE. *A Study in Modern Superstition.*

By Jacques Barzun. Harcourt, Brace and Co., New York. $2.50. 8 x 5⅜; x + 353; 1937.

In essence, this work is an exposition of the controversies which in France have taken place over the question of race and racial superiority. The author wishes to show that this question brought to the front in Germany today is not an original product of Nazism but that at different periods, in France as well as other countries, it has been the subject of grave and animated discussion by philosophers, writers and scientists. In demonstrating that this belief in "pure" race, racial superiority, etc. is simply a superstition the author tells us nothing new, but in assuming that the disquisitions of the intellectuals are of great weight either in fostering or maintaining this belief he overlooks the most important elements in this phenomenon—the emotional reaction which it arouses in the population group. Moreover, in attempting to show that the question of race has really no scientific meaning the author proceeds to a wholesale indictment and ridicule of the findings of physical anthropology and of the vital and health statistics with regard to "racial" differences. This is a smart journalistic device but leaves doubts as to the author's seriousness of purpose or profundity of knowledge. There is an extensive if not complete bibliography on the subject.

TOTEMICA. *A Supplement to Totemism and Exogamy.*

By Sir James G. Frazer. The Macmillan Co., New York and London. $6.50. 8½ x 5½; xii + 518; 1938.

As the sub-title indicates, Frazer here has compiled some of the principal observations concerning totemism made since the publication in 1910 of his monumental treatise. Approximately half of the book is dedicated to a review of the data on Australia. The importance given the observations on the native Australian is due essentially to the fact that more information has been collected about these peoples than about any other and because

from the varied forms in which totemism is manifest among them it is hoped to find a clue to its origin. Besides totemism in Australia, the author reports on the observations made in Melanesia, New Guinea, India, Africa and North America. It is to be noted that in this book more emphasis is placed on the facts than on possible interpretations or generalizations. This is a happy reversal of the method adopted by some of the more enthusiastic of Frazer's disciples. Since the author fails to summarize the observations reported here or comment on their bearing to his earlier conclusions, we hope that in the near future this omission will be remedied by a mature summation and evaluation of the significance of this complex social phenomenon.

NOT SO LONG AGO. *A Chronicle of Medicine and Doctors in Colonial Philadelphia. By Cecil K. Drinker. Oxford University Press, New York.* $3.50. 8½ x 5⅝; xii + 183 + 12 plates; 1937.

Professor Drinker here presents extracts and notes from the diary which his great-great-grandmother, Elizabeth Drinker of Philadelphia, kept from 1758 to 1807. The items which he has selected deal in particular with the methods of hygiene and medical practice in vogue in that period. Elizabeth Drinker, it is apparent, was a shrewd and intelligent observer of the life around her. Moreover she took an active interest in medical questions especially when related to the health of her family and was able to avail herself of the best professional care which that period could give. From her diary the reader acquires a vivid picture of the bathing and toilet facilities in the homes of that time and the care and treatment for childbirth, tuberculosis, smallpox, and yellow fever. In addition, there are given interesting pen-portraits of such eminent physicians as Rush, Shippen, Bard, Kuhn and Physick. As noted in the title, the conditions described were present not so long ago, a matter of only 140 years. But this was an interval of progress in public health and sanitation so great that it takes a vivid account like this to give us a true realization of its import.

DR. NASH'S COOKERY BOOK. *By Elwin H. T. Nash. Simpkin, Marshall, London.* 6d. net. 8½ x 5½; 183; 1937 (paper).

This cook book, issued by an English doctor (whose hobby has long been cooking) furnishes to the family of small income an excellent series of recipes which, if followed with care, will produce palatable dishes with an adequate protein ration at a very low cost. An intelligent and enthusiastic group of workers put in a year's research in experimenting and testing. "Some of these dishes were altered as many as ten times until the right flavour or consistency was obtained. Throughout the researches the price paid for beef averaged 6d. per pound, and for mutton, 3d. per pound." Always it was kept in mind "that the smaller wage earner, speaking generally, likes his flavours more pronounced than other people." If any criticism is to be made, it is that so little space (3 pages only) is devoted to the cooking of vegetables—yet this is not altogether a just criticism for vegetables are used repeatedly in the meat dishes. Being a cookery book, it does not touch upon the many ways in which raw vegetables and fruits can be used. If this reviewer lived in England he would certainly obtain a copy of this book and study it carefully. The chutney recipes are already being tested in our American kitchen.

NOTES ON A DRUM. *Travel Sketches in Guatemala. By Joseph H. Jackson. The Macmillan Co., New York.* $3.00. 8 x 5½; x + 276; 1937.

Because the author's room in Guatemala contained no table but a tall Mayan drum, upon this he wrote his notes on the surrounding country and its people. Hence the title of the book. Mr. Jackson having, like all good tourists, carefully

read up on the country and its history, had fairly definite plans as to what he wanted to see. These he submitted to the Clark Tours people who skillfully and economically arranged his travels in Guatemala. This volume, however, is more than a helpful guide for future travelers to this interesting country, for it is written in an amusing, informal style and is worth reading for entertainment alone. Sixty full page photographs taken by the author enrich the book, giving the reader a more complete idea of the country and its inhabitants. Here and there some of the past history of the country is brought in, making it more interesting for the modern explorer. Unfortunately he has not included a bibliography and there is neither an index or a table of contents.

AFRICAN GENESIS.

By Leo Frobenius and Douglas C. Fox. Stackpole Sons, New York. $3.00. 9 x 6; 236 + 6 plates; 1937.

Because Frobenius' rock picture research led him to believe there might be a transfusion of cultures and faiths between African and Egyptian civilizations, he undertook to investigate the mythology back of these paintings. This body of folklore, the substance of this volume, makes delightful, entertaining and refreshing reading. The legends, collected from a number of African tribes are written in clear straightforward language which is almost poetical in its simplicity of style and idea. The book is illustrated with drawings from rock paintings taken from the different parts of Africa explored by the Frobenius expedition. These have nothing to do with the actual body of the text. However, Fox, in his foreword in which he theorizes about the meaning and origin of these legends, attempts to explain the paintings by the legends. In addition there are a number of extremely well done pencil portraits of the African natives and an occasional map showing the areas in which the rock pictures were collected.

THE AMERICAN INDIAN. *An Introduction to the Anthropology of the New World. Third Edition.*

By Clark Wissler. Oxford University Press, New York and London. $3.75. 8½ x 5⅝; xvii + 466 + 9 plates + 1 folding map; 1938.

Dr. Wissler's book can be considered the Bible of New World anthropologists. Starting off with the distribution of the aborigines and their social patterns as determined by the food areas of the continent, he proceeds into the cultural traits of the various tribes. Instead of penetrating any one field thoroughly, the author lightly covers all the phases of culture in a concise manner, but giving sufficient information to enable the reader to gain a conception of the extent to which the Amerinds had progressed. The origin of the American Indian is also discussed. There is appended a classification of the linguistic stocks of the United States and Canada and a bibliography of 32 pages. The tyro or the well-versed scholar will find information, both interesting and new, whether it concerns the Abipones or the Zuni, in this revised edition.

A BIBLIOGRAPHY OF THE WORKS OF AMBROISE PARÉ: *Premier Chirurgien and Conseiller du Roy.*

By Janet Doe. University of Chicago Press, Chicago. $5.00. 10¼ x 7¼; xx + 266 + 22 plates; 1937.

For the past century Malgaigne's bibliography has served as the authority on the writings of the sixteenth century surgeon, Paré. However, it covers only the editions extant in Paris in 1840, together with those listed by a previous bibliographer, Haller, and contains several mistaken entries copied from previous writers. Miss Doe has now compiled all the available information concerning the editions of Paré's works, including six editions previously unknown to bibliographers, corrected the errors of Malgaigne's list, and furthermore added interesting little digressions into the surgeon's life and personality, his patrons, publishers,

friends and enemies. Thus this book is more than a cut and dried notation of items. Extracts from the books are cited and numerous facsimile pages serve as illustrations.

THE LAND OF THE GURKHAS *or the Himalayan Kingdom of Nepal.*

By W. Brook Northey. With a chapter by the Hon. C. G. Bruce. W. Heffer and Sons, Cambridge. 10s. 6d. net. $8\frac{3}{4}$ x $6\frac{1}{8}$; x + 248 + 66 plates + 1 folding map; 1937.

Since there is relatively little information available regarding the kingdom of Nepal and its people, this account written by an officer with 20 years experience in a Gurkha regiment is one that will interest the human biologist and whet his curiosity about this land practically closed to the ordinary traveller. In this volume will be found a comprehensive although necessarily condensed description of the geography of Nepal, of the customs and arts observed in its principal centers and of the history of the Gurkha. The author is apparently not interested either in the ethnology or anthropology of the country and consequently the reader who hopes to obtain a picture of the conditions of life and civilization of Nepal will be disappointed. However, even with these defects, the book will be found interesting and worth delving into for what it gives on the subject.

THE FUTURE OF CIVILISATION AND SOCIAL SCIENCE. *A Study Involving the Principles of Scientific Meliorism.*

By W. P. Dreaper. E. T. Heron and Co., London. 1s. $7\frac{1}{4}$ x $5\frac{1}{4}$; 48; 1937 (paper).

Scientific Meliorism, a humanitarian form of socialism, was proposed by Jane Hume Clapperton in 1885 as a means of attaining the ideals of goodness which she believed to be the goal of man's evolution. The theory, as advocated by her, was essentially a general application of the methods so dear to the heart of the Victorian, "to repress in the young certain instincts which are natural and therefore unblamable." In restating and reviving Jane Clapperton's ideas, Mr. Dreaper is apparently unaware that nowadays those interested in the study of society are not receptive to vague theories which are as far from reality as the characters of an Elsie Dinsmore book. It might be added that the 'scientific' in Scientific Meliorism has about the same value as the 'science' in Christian Science.

A HISTORY OF WOMEN IN MEDICINE *From the Earliest Times to the Beginning of the Nineteenth Century.*

By Kate Campbell Hurd-Mead. The Haddam Press, Haddam, Conn. $6.00. 9 x 6; xvi + 569 + 60 plates; 1938.

This is the first of two volumes which together will adequately reveal for the first time the place of women in the history of medicine. This volume carries the story from the earliest times (beginning in Egypt, Assyria, and Palestine), comes down through Greece and Rome to mediaeval Europe, and then includes the various countries of the world up to the beginning of the nineteenth century. The second volume, to be published in the near future, will carry the story up to date. An admirable work is presented here both as to the subject, neglected until now, and as to the diligence in personal research throughout all the different sources in most of the great libraries in Europe. It is indeed a lifetime study that began forty years ago. Every page is interestingly and fascinatingly written and numerous quotations from the original sources are included. Naturally it will be of great interest to the historian of medicine. The index covers more than fifty pages.

MEDICAL CLASSICS. *Volume I, Numbers 2 to 10.*

Compiled by Emerson Crosby Kelly. Williams & Wilkins Co., Baltimore. Subscription price $10.00 per volume; single copies $1.25. 10 x 7; 81–928; 1936–1937 (paper).

These numbers complete Vol. 1 of this

series of publications. Similar to the first number (cf. Q. R. B., Vol. 12, p. 240), each of these presents a brief biographical sketch of the author whose work is extracted or reprinted, bibliographic data, and a note regarding the influence and significance of the work given. Each of the numbers contains reprints of one or more articles or selections from the writings of one of the following authors: Charles Bell, Oliver Wendell Holmes, Percivall Pott, Theobald Smith, Dominic J. Corrigan, Thomas Hodgkin, Nathan Smith, Douglas Argyll Robertson and Guido Banti. As the reader will note, the names of the majority of these authors are associated with disease entities regarding the clinical description of which precious little has been added in recent years. It seems almost superfluous to say that a careful reading of these papers will interest and benefit not only the physician flut also the biologist.

FIFTH AVENUE TO FARM. *A Biological Approach to the Problem of the Survival of Our Civilization.*

By Frank Fritts and Ralph W. Gwinn. Harper and Bros., New York and London. $3.00. 8¼ x 5⅝; viii + 282; 1938.

Belief that American civilization is becoming genetically inferior, due to migration of the superior individuals from farms to cities, is the theme of this book. These individuals then fail to reproduce themselves, while the inferior persons left on the farms are the reproducers of the population. The proposal is made that country life could be very attractive to the biologically more desirable elements, and that a back to the farm movement should be stimulated. Granted that the country is, or could be, an attractive place for anyone to live, nevertheless the authors are treading on thin ice in assuming superiority or inferiority and its transmission in such extensive groups as urban and rural populations.

THE MARGINAL MAN. *A Study of Personality and Culture Conflict.*

By Everett V. Stonequist. Charles Scribner's Sons, New York. $1.60. 7¾ x 5⅛; xviii + 228; 1937.

By marginal man is here meant the individual who either through heritage or migration is brought into close contact with two different and often antagonistic types of civilizations and cannot participate completely in either. Thus the author examines the problems of the descendants of some of the race (color) marriage mixtures of India, South Africa, United States, Java, Hawaii and Brazil. He discusses also the problems and methods of adjustments of the European immigrants in the United States and of their offspring, and of the so-called racial minorities in Europe. As a source of bibliographic references this book will be found useful, but otherwise it is a somewhat diffuse compilation that demonstrates very little originality except in chapter and section headings.

RASSENKUNDE UND RASSENGESCHICHTE DER MENSCHHEIT. *Zweite umgearbeitete und erweiterte Auflage in zwei Bänden. Erster Band. Die Forschung am Menschen. Erste Lieferung (Bogen 1–8). Zweite Lieferung (Bogen 9–16). Dritte Lieferung (Bogen 17–22).*

By Egon F. von Eickstedt. Ferdinand Enke Verlag, Stuttgart. Bogen 1–8, RM. 8.; Bogen 9–16, RM. 8.; Bogen 17–22, RM. 6.60 (25 per cent less outside of Germany). 10¼ x 7; 352; 1937–38 (paper).

The first edition of this monumental history of the races of mankind appeared in 1934. The necessity for a new edition after only three years is a good sign of the significance and popularity of the work. In the three numbers now at hand an introduction into the fundamentals of anthropology and the history of research on man since antiquity is given in connection with numerous neighboring points from biology, geography, history, and the arts. The racial standpoint of modern Germany and the Nordic idealogy of Gobineau and Chamberlain receive relatively brief treatment in comparison with the broad quotations from other literature. Each number is extensively documented.

FAMILY LIVING IN KNOTT COUNTY, KY. *U. S. Department of Agriculture. Technical Bulletin No. 576.*

By Faith M. Williams, Hazel K. Stiebeling, Idella G. Swisher and Gertrude S. Weiss. Government Printing Office, Washington. 10 cents. 9¼ x 5⅞; 69 + 1 plate; 1937 (paper).

This study is part of a comprehensive survey of economic and social conditions in the southern Appalachians, and attempts to show the effect of farming upon material conditions of family life. It was found that the level of living of the 228 families studied was, on the whole, lower than that of many groups of farm families in other areas. The authors conclude that the problems of the people of this section call for economic action on a greater scale than the individual family can undertake. There is a bibliography and an appendix containing a classification of the goods and services entering into the value of current family living.

PRELIMINARY REPORT ON THE SMITHSONIAN INSTITUTION-HARVARD UNIVERSITY ARCHEOLOGICAL EXPEDITION TO NORTHWESTERN HONDURAS, 1936. *Smithsonian Miscellaneous Collections, Vol. 97, No. 1. (Publication 3445).*

By William D. Strong, Alfred Kidder II and A. J. Drexel Paul, Jr. Smithsonian Institution, Washington. 80 cents. 9¾ x 6¾; v + 129 + 16 plates; 1938 (paper).

This progress report on excavations of mounds in the valley of the Ulua River, in 1936, describes fragments of pottery made by Maya, Jacaque, and Lenca peoples in pre-Columbian times and discusses their usefulness in tracing cultural trends. Potsherds and reconstructed vessels and figurines are described in considerable detail and illustrated by a large number of drawings and photographs. Some descriptions of the inhabitants of the region, written by early Spanish explorers and colonists, are presented in translation.

PHYSICIANS OF THE MAYO CLINIC AND THE MAYO FOUNDATION.

Division of Publications, The Mayo Clinic. University of Minnesota Press, Minneapolis; Oxford University Press, London. $10.00. 9¼ x 6¼; vi + 1575; 1937.

This volume is divided into four sections: (1) a main section containing biographies, in alphabetic order, and bibliographies, in chronological order, of persons who served in the Mayo Clinic or Foundation for one year or more; (2) a section containing brief biographies of persons who were officially connected with the Clinic or Foundation for less than a year; (3) an index of universities or colleges from which persons appearing in the main section received their degrees; (4) a geographic index of the scientific workers from all over the world who have been connected with the Rochester clinic.

THE HERVEY ISLANDS ADZES *in the Peabody Museum of Salem.*

By Ernest S. Dodge. Peabody Museum, Salem, Mass. 75 cents. 10 x 7½; iv + 16 + 13 plates; 1937 (paper).

This is the second of a series of catalogues to be published by the Peabody Museum of Salem on its collection of Polynesian ethnological material. The present publication contains illustrations and descriptions of thirty-two specimens of a single type of object—the adz—an instrument of questionable purpose, consisting of an ax-like blade set in a handle which is often ponderous and usually elaborately carved.

BERICHT DER 12. VERSAMMLUNG DER INTERNATIONALEN FEDERATION EUGENISCHER ORGANISATIONEN. *Konferenzsitzungen vom 15. bis 20. Juli 1936. Scheveningen, Holland.*

W. P. Van Stockum and Zn. N.V., The Hague, Holland. Flor. 3. 9¾ x 6½; 119; 1937 (paper).

This volume of proceedings contains 25 papers, published either *in toto* or in abstract, on race hygiene, sterilization laws, mutation and other topics of interest to eugenists. Two of the papers are in French, the others, about equally divided, in English or German. The articles are preceded by a list of members of the International Federation of Eugenic

Organizations, and followed by reports of committees.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 468. Abt. VII, Methoden der vergleichenden morphologischen Forschung, Teil 2, Heft 3.* Containing the following article: *Spezielle Methoden anthropologischer Messung*, by Theodor Mollison. *Urban and Schwarzenberg, Berlin.* RM. 9. 10 x 7; 160; 1938 (paper).

This is a description of the most important methods of anthropological measurements on the living subject and on the skeleton, including instruments and graphical representation. It concludes with a brief statistical treatment of results. It is provided with numerous illustrations and a bibliography.

A GRAPHIC SUMMARY OF THE NUMBER, SIZE, AND TYPE OF FARM, AND VALUE OF PRODUCTIONS (*Based Largely on the Census of 1930 and 1935*). *U. S. Department of Agriculture, Miscellaneous Publication No. 266.*

By O. E. Baker. Government Printing Office, Washington. 10 cents. 9¼ x 5⅞; 76; 1937 (paper).

For a brief bird's-eye view of the status of agriculture in the United States today, this bulletin has been excellently prepared. The increase or decrease in number and size of the different types of farms, together with the value of products are shown graphically by map diagrams. The data are taken largely from the census reports of 1930 and 1935.

ZOÖLOGY

THE BIRDS OF AMERICA.

By John James Audubon. With an Introduction and Descriptive Text by William Vogt. The Macmillan Co., New York. $12.50. 12½ x 9; xxvi + 500 plates; 1937.

The reputation of this classical work renders any extensive detailed comment unnecessary. The current edition commemorates the centennial anniversary of the original publication, and contains sixty-five additional illustrations of birds from the Pacific slope, which Audubon painted at a later date. The original edition of *The Birds of America* was published in London in the years 1827–38. Each plate was reproduced from an original water-color painting by Audubon. The process used for reproduction was aquatint engraving on copper plates, with hand coloring of the impressions. The plates were printed on Whatman paper of double elephant folio size 39½ x 26½ inches before trimming. There were 435 of these plates. The set was priced at £ 182/14 in England, and $1000 in the United States. In 1840–44 an octavo edition (lithographed and hand-colored) in seven volumes was published in New York and Philadelphia.

The present work is the first edition of these famous plates ever to be issued in a single volume. And it is a superb piece of book-making. The plates are reproduced by lithography and on the whole extraordinarily well. At the price the book is a real bargain.

The brief texts accompanying each plate, while necessarily very condensed have been extremely well done by William Vogt. Ranges, habitats, and conspicuous diagnostic characters are indicated.

Altogether we congratulate the publishers on the fine service they have done biology in producing this book.

THE CALL OF THE KOALA.

By Ambrose Pratt. Robertson and Mullens, Melbourne. 6s. 8½ x 5½; 120 + 18 plates; 1937.

KOALA. *The story of Australia's Native Bear.*

By Charles Barrett. Robertson and Mullens, Melbourne. 2s. 8½ x 5½; 31 + 15 plates; 1937 (paper).

When we take cognizance of the fact that in the early twenties of the present century as many as 1,500,000 Koala pelts were exported from Australia to Britain alone per annum, and that today the entire

race of Koalas numbers less than 1,000, we realize and appreciate how nearly this "native bear" came to the fate of our own American passenger pigeon—extinction. It is for the purpose of conservation, and appreciation of what this popular little animal has to offer to the world of science as well as to the entire nature-loving world, that these two little books have been written.

The fourteen years of study which have led the author of the first volume into every phase of the Koala's life have made him well qualified to write on the subject. Pratt's work has included detailed studies of the anatomy, physiology, classification, origin, and food of Australia's "native bear." Particularly has he been interested in determining the cause and discovering a method of preventing the high death rate among bears both in captivity and in the free state. The work has presented almost unsurmountable difficulties in that (1) it is pioneer in nature, and (2) it deals with an organism that is almost entirely incapable of adapting itself to conditions outside the narrow realm in which it has lived practically unchanged for the past million years. This little volume is truly a scientific treatise, written in a popular style, and with a note of sympathy that appeals to every nature lover.

The second volume deals with the early history of the Koala, its classification, habits and value, together with the effect of colonization of its native Australia on its natural population.

Both books include a number of excellent photographs of the Koala in some of its most characteristic poses. The former includes several photos of the different species of *Eucalyptus*, the leaves of which make up the entire diet of this strange little marsupial.

SILVER FOX PELT PRICES AS AFFECTED BY TIME OF PELTING, SEX, AND AGE. *U. S. Department of Agriculture, Circular No. 460. By Chas. E. Kellogg. Government Printing Office, Washington.* 10 cents. 9¼ x 6; 27 + 2 plates; 1937 (paper).

The industry of silver fox fur farming is of such recent origin that factual information concerning its many phases has not yet been collected to any great extent. Statistics concerning the effect of time of pelting, sex, age, degree of silver, etc., on the value of pelts have been needed for the obvious purpose of putting the industry on a more scientific and profitable basis.

The Bureau of Biological Survey of the Department of Agriculture has made a study of pelt values based on the industry of the Herbert A. Nieman Co., Thiensville, Wis., and has published the findings in this bulletin. The data concerning the value of the furs were collected by tabulating the prices received for some 10,000 pelts sold by the New York Auction Co. in November, December, January, and February of 1935–36. In calculating the mean value for furs of different types sold each week during the period, it is assume that the quality of the pelt alone determines the price received, and no mention is made of the possible effect of supply and demand on the variability of price.

THE FOOD OF NORTH SEA HERRING 1930–1934. *Fishery Investigations, Series II. Vol. XV. No. 5. By R. E. Savage. His Majesty's Stationery Office, London; British Library of Information, 270 Madison Ave., New York.* 95 cents. 10¾ x 7¼; 57; 1937 (paper).

MARINE FAUNA OF THE ISLE OF MAN. *Reprinted from "Proceedings and Transactions of the Liverpool Biological Society," Volume 50. Compiled by Hilary B. Moore. Edited by R. J. Daniel assisted by J. R. Bruce and M. W. Parke. University Press of Liverpool, Liverpool.* 2s. net. 8½ x 6¼; 293; 1937 (paper).

Two selected areas of the North Sea were used as collecting grounds for the 34,124 herring whose stomach contents were examined in this study. The principal organisms present in the food of the herring and their prevalence and sequence during stated intervals of time were noted, the percentage composition of the food calculated and the total volume of the food ascertained. Comparisons were made

of annual variations in the quantity and type of food consumed and comparative estimates made to show the relative quantity of food taken out of the water each season. Twelve reference works are listed in the bibliography.

Old and new data have been brought together in *Marine Fauna of the Isle of Man* in the preparation of a classified list of the marine animals found on the shores of this island between tide marks, and in the surrounding sea to a distance of about five miles. An up-to-date record of the distribution of these forms is included. Three charts show the location of the various collecting grounds, all of which are briefly described in the text with the exception of the Dub Reef of Port Erin Bay for which a detailed account of the fauna is given. The book includes an index and both reference and record bibliographies.

GIANT FISHES, WHALES AND DOLPHINS. *By J. R. Norman and F. C. Fraser. Illustrated by Lieut. Col. W. P. C. Tenison. W. W. Norton and Co., New York.* $4.00. 8½ x 5½; xxvii + 341 + 8 plates; 1938.

This book is the scientific reply to the many inquiring sportsmen, travelers, and seafaring men who, for sundry reasons, desire information concerning whales, dolphins, and larger kinds of fish. To merit a place in this work Neptunian members must possess a physical combination of true backbone and six feet of length. The only ones to qualify were the six-foot sharks, rays, bony fishes, whales, dolphins, and porpoises. The lampreys and hagfishes were omitted, and such enormously long members of the Coelenterata, Mollusca, and Arthropoda as are popularly called "fishes" were also excluded for lack of a qualifying backbone.

The book is divided into two parts. Part I deals with the Selachii and Pisces; Part II, with the Cetaceans. Conversationally written in generally nontechnical language and arranged for facile, ready reference, this work is both a scientific and a picturesque presentation of its subject. History, legend, and personal anecdotes pleasantly flavor scientific descriptions of the breeding habits, diet, habitat, structure and appearance of the selected creatures. Simple keys for identification are also included. There are 8 colored plates, 97 drawings for identification purposes, and other numerous quaint historical and legendary sketches.

ATLAS OF THE SCALE INSECTS OF NORTH AMERICA. *By G. F. Ferris. Stanford University Press, Stanford University; Oxford University Press, London.* $8.75. 10¾ x 8½; 8 + [544]; 1937.

The ambitious purpose of this series (of which this is only the first part) is "to make possible the definite identification of every species of scale insect now known from North America." To make this project feasible the book is in atlas form and is essentially a collection of plates of the different species with a minimum of textual material. One can grasp the magnitude of the problem from the fact that this first series covers only the Tribe Diaspidini of the Family Diaspididae; the latter is only one of the 11 families in the Coccoidea. The scale insects are a confusing group, and at the same time their importance to man is so great that this monograph is doubly welcome. Economic entomologists will find this series indispensable.

GREEK WOLF-LORE. *A Dissertation in Greek Presented to the Faculty of the Graduate School of the University of Pennsylvania in Partial Fulfillment of the Requirements for the Degree of Doctor of Philosophy. By Richard P. Eckels. (Small number of copies for free distribution by the author, 1 N. Harrisburg St., Steelton, Pa.)* 9¼ x 6½ inches; 88, 1937 (paper).

A doctoral dissertation setting forth and discussing what the ancients, particularly the Greeks, knew or did not know about the wolf; beliefs and superstitions about him and something of their later history; the werewolf and 'lycanthropy' (medical); the place of the wolf in certain Greek

cults; and the wolf as nurse, with special reference to the she-wolf who suckled Romulus and Remus.

The significant point is that, where the ancients had numerous superstitions and fallacious ideas about wolves, we moderns have only a few (e.g., that wolves run in packs and that they attack men out of hunger or even out of malice) and those few quite as erroneous and as staunchly believed in as those held as articles of faith in bygone days.

Indices of the source material and of the authors cited and referred to are provided. The treatise will interest not only classicists and folklorists but also historians of science and of medicine.

A Monograph of the Acanthodriline Earthworms of South Africa.

By Grace E. Pickford. W. Heffer and Sons, Cambridge. 25s. net. 9¾ x 6; 612 + 2 folding charts; 1937.

A good many years went into the making of this comprehensive survey of the Acanthodrilinae of South Africa. Where they had already been studied (viz., scattered localities in the region of Port Elizabeth, Cape Peninsula and the Knysna Forest, and a few places in Natal and the Transvaal) they exhibited a high degree of local endemicity.

It was the purpose of the present investigation to study the unknown intervening regions with a view to elucidating this problem of local endemicity in the light of past and present geographical and climatic conditions. It is hoped that the preliminary results so obtained may form the basis for some more complete survey by future investigators.

A valuable reference work. It is abundantly illustrated and has an appendix of taxonomic notes on non-South African genera and species re-examined in connection with the present investigation, and an alphabetical index to the South African species of Acanthodrilinae. Unfortunately there is no general index.

A Nature Lover in British Columbia.

By H. J. Parham. H. F. and G. Witherby, London. 8s. 6d. net. 8½ x 5½; 292 + 21 plates; 1937.

Soon after the author settled in southern British Columbia, he began to observe the wild life about him and before long knew most of the birds, mammals, and fish of the region. In a pleasantly rambling fashion he relates experiences that show keen observational powers. He is greatly vexed by the lack of protection afforded the wild life by the government and he devotes an entire chapter to the methods by which the birds might be saved through governmental aid. The author unfortunately digresses in the last chapter entitled "The search for truth," wherein he expounds his philosophy of life. Otherwise the book is very entertaining and enjoyable to read. There is an index and several appendices that list the mammals, birds, fish, and plants of the Okanagan Valley with their scientific names.

The Biological Control of an Insect in Fiji. *An Account of the Coconut Leaf-mining Beetle and its Parasite Complex.*

By T. H. C. Taylor. Imperial Institute of Entomology, London. 12s. net. 9¾ x 6⅛; 239 + 23 plates; 1937.

This smoothly-written account of the successful control of the leaf-mining beetle *Promecotheca reichei* Baly in Fiji by biological means begins with an account of the life history of the pest and of its principal parasites. Oddly enough, one of its parasites, the mite *Pediculoides ventricosus* Newp., complicated the problem by "periodic elimination of all developmental stages, except one, of *P. reichei* itself," thus depriving the other parasites of one of their principal sources of food for a large part of the year. The practical details of selecting and rearing parasites and estimating their effect on the pest are described and the principles of biological control are concisely stated. The book is well illustrated with photographs and drawings of the insects discussed. There is no index.

Zoologica. *Scientific Contributions of the New York Zoological Society, Volume XXII, Part 4, Numbers 21–28.*

New York Zoological Society. Zoologica

Park, New York. $1.80. 10½ x 7; 111 + 3 plates; 1937 (paper).

In this issue of *Zoologica* the first paper is on the histological structure of the normal and the hyperplastic thyroid in *Rasbora lateristriata* (Bleeker), a small tropical fish. Serial sections (3 plates) show that the excessive growth of the thyroid destroys and replaces the gill structures. The other papers are on lymphocystis in the hogfish *Lachnolaimus maximus*, on the "display" of the Twelve-wired Bird of Paradise and of the Magnificent Rifle Bird, on decapod crustacea from the Lower California region, on deep-sea fishes of the Bermuda oceanographic expedition [Serrivomeridae (genus *Platuronides*) and Nemichthyidae], and the caudal skeleton of Bermuda shallow water fishes (order Iniomi: Synodontidae). All of the papers have text figures.

HORMONE BEI WIRBELLOSEN TIEREN. *Probleme der Biologie, Band I.*

By Gottfried Koller. Akademische Verlagsgesellschaft M.B.H., Leipzig. RM. 9.20 (paper); RM. 10.80 (cloth). 8¼ x 5¾; viii + 143; 1938.

This little book is a summary of all that is known on the subject of hormones among invertebrates. The author in defining his concept of hormones points out that recent work has shown that there are many stuffs in organisms that have peculiarities in common with hormones but cannot be said to be glandular products. These stuffs are discussed under the following chapter headings: Cell hormones; Aglandular tissue hormones; Glandular tissue hormones; Hormones producing color changes; Glands of internal secretion among molluscs and tunicates.

BATTLES WITH MONSTERS OF THE SEA.

By F. A. Mitchell-Hedges. D. Appleton-Century Co., New York. $4.00. 8¾ x 6½; x + 349 + 24 plates; 1937.

This is a lively narrative of fishing thrills off the coast of British Honduras and among the islands off the coast of the Republic of Honduras and in the Gulf of Panama. Although the author was collecting all the time, this is mainly a story of adventure, excitement and hair-breadth escapes. The captures include a 960 pound jewfish, a 1,960 pound tiger shark, a 4,600 pound saw-fish and an eagle-ray weighing 5,200 pounds. For those interested in adventure and the endless bounties of southern waters and tropical islands, this book with its pleasant style should bring great enjoyment. Many clear photographs accompany the text and there is a complete index.

LIFE OF THE OCEAN (FISHES, ETC.). *Wonders of the Sea. Art and Nature in Colour Series.*

Introduction by E. G. Boulenger and A. Portmann. Plates painted from nature by Paul A. Robert. B. T. Batsford, London. 7s. 6d. net. 13¾ x 10⅛; 8 + 12 plates; 1938 (paper).

This volume is another number in a series dealing with "Art and Nature in Colour," which has already been noticed in these columns. The compilers have gone to the bottom of the sea for their material. The beautifully colored illustrations are replete with forms of animal and vegetable life which quickly lose their color when removed from their natural environment, and with which consequently, the majority of people have no first hand acquaintance. The book is more likely to appeal to the artist than to the scientist.

WORLD NATURAL HISTORY.

By E. G. Boulenger. With an Introduction by H. G. Wells. Charles Scribner's Sons, New York; B. T. Batsford, London. $3.00. 8½ x 5½; xx + 268 + 150 plates; 1938.

While extremely condensed—so that the Hymenoptera for example get only a trifle over two pages—it seems fair to say that for its size this is the best general natural history (dealing only with animals) yet to appear. The illustrations are superb. Anyone wanting to put in the hands of his children a general conspectus of animal life cannot do better than to give them this

book. Incidentally they will learn a lot about evolution.

AXIAL BIFURCATION IN SERPENTS. *An Historical Survey of Serpent Monsters Having Part of the Axial Skeleton Duplicated.*

By Bert Cunningham. Duke University Press, Durham, N. C. $2.50. 9 x 6⅛; vii + 117; 1937.

For anyone interested in ophidian teratology, this book will furnish extensive information. It cites all records that have ever been reported, including historical accounts, on dichotomy in serpents, and whenever possible gives an illustration of the specimen.

There is a bibliography, indices of authors and specimens, and 12 plates consisting of photographs and X-rays of recent material, and photographic copies of the earlier reproductions.

SWIFT MOVEMENT IN THE TREES *and at Their Roots.*

By Phyllis Kelway. Longmans, Green and Co., New York. $2.50. 7⅞ x 5⅝; x + 189 + 15 plates; 1937.

These are stories of Miss Kelway's charmingly intimate experiences with her animal friends including squirrels both red and grey, shrews, moorhens, and her pet toads, Geraldine, Boadicaea, and the lively Natterjack. Although the stories themselves are of little scientific interest, her numerous photographs of these wild creatures are excellent. Her way of interpreting their feelings, their psychology and their language is nothing short of amazing.

THE OXYSTOMATOUS AND ALLIED CRABS OF AMERICA. *United States National Museum Bulletin* 166.

By Mary J. Rathbun. Government Printing Office, Washington. 60 cents. 9⅛ x 6¼; vi + 278 + 86 plates; 1937 (paper).

This publication is a continuation of Dr. Rathbun's work on the crabs of America. It is the fourth of the series and it maintains the fine standards of workmanship that characterized the previous volumes. Each species is described, the range and measurements given, and usually a table of the material examined by the author is included. There are many pen and ink drawings and 86 plates of photographs.

AFRICAN HUNTER.

By Bror von Blixen-Finecke. Translated from the Swedish by F. H. Lyon. Alfred A. Knopf, New York. $2.75. 7½ x 5½; [8] + 284 + viii + 22 plates; 1938.

Game laws and licenses, characteristics of the natives, and wild animal traits all interest one in this book more than the courageous deeds of the author or his associations with the elite. Changes in the characteristics and supervision of Africa during the past twenty-five years are well depicted. The volume is indexed and contains many good photographs.

THE SNAKES OF NEW JERSEY, *A Guide.*

By Harold Trapido. The Newark Museum, Newark, N. J. 60 cents. 9⅛ x 6; 60; 1937 (paper).

This is a guide to the 21 species of snakes known to occur in New Jersey. There is a key to the species, each of which is illustrated by a photograph, with notes on the range, occurrence in New Jersey, habitats, and habits. The pamphlet will be extremely useful to natural history students.

DIE BLATT-MINEN MITTEL- UND NORD-EUROPAS EINSCHLIESSLICH ENGLANDS. *Bestimmungs-Tabellen aller von Insekten-Larven der verschiedenen Ordnungen erzeugten Minen. Lieferung 6.*

By Martin Hering. Gustav Feller, Neubrandenburg. Subscription price for 6 numbers: (Germany and Switzerland) 12 marks; (foreign, except Switzerland) 9 marks. 9¼ x 6¼; 561–631; 1937 (paper).

The concluding number of an alphabetized list of plants attacked by leaf-miners. This contains additions to the list of

plants, correction of errors, classification of the plants and insects considered, and an index of these insects. Preceding issues are reviewed in volumes 11 and 12 of Q. R. B.

THIRD LIST OF ANTILLEAN REPTILES AND AMPHIBIANS. *Bulletin of the Museum of Comparative Zoölogy at Harvard College. Volume LXXXII, No. 2.*

By Thomas Barbour. Museum of Comparative Zoology, Cambridge. 75 cents. 9½ x 6¼; 92; 1937 (paper).

Recent work has necessitated the publication of a third list of the Antillean reptiles and amphibians. The forms listed from this region now number 483.

A STATISTICAL STUDY OF THE RATTLESNAKES. *IV. The Growth of the Rattlesnake. Occasional Papers, San Diego Society of Natural History, No. 3.*

By Laurence M. Klauber. San Diego Society of Natural History, San Diego, Cal. 8½ x 5½; 56; 1937 (paper).

The data used in the analysis comprise body lengths and weights, growth measurements, number in brood, snake den populations, etc. Tables and graphs have been included.

A NEW SNAKE OF THE GENUS SONORA FROM MEXICO. *Transactions of the San Diego Society of Natural History, Vol. 8, No. 27.*

By Laurence M. Klauber. Society of Natural History, San Diego, Calif. 10⅛ x 6⅝; 4; 1937 (paper).

A NEW SEA-URCHIN FROM THE "OLIGOCENE" OF OREGON. *Transactions of the San Diego Society of Natural History, Vol. 8, No. 28.*

By Hubert L. Clark. Society of Natural History, San Diego, Calif. 10⅛ x 6⅝; 8 + 1 plate; 1937 (paper).

AN EXTINCT PUFFIN FROM THE PLIOCENE OF SAN DIEGO, CALIFORNIA. *Transactions of the San Diego Society of Natural History, Vol. 8, No. 29.*

By Loye Miller. Society of Natural History, San Diego, Calif. 10⅛ x 6⅝; 4; 1937 (paper).

AN UPPER PLEISTOCENE FAUNA FROM THE BALDWIN HILLS, LOS ANGELES COUNTY, CALIFORNIA. *Transactions of the San Diego Society of Natural History, Vol. 8, No. 30.*

By George Willett. Society of Natural History, San Diego, Calif. 10⅛ x 6⅝; 28 + 2 plates; 1937 (paper).

NOTES ON THE DEVELOPMENT OF TWO YOUNG BLUE JAYS (CYANOCITTA CRISTATA). *By A. L. Rand. Proceedings of the Linnaean Society of New York,* No. 48: 27–58, 1936 (1937).

BOTANY

RANGE PLANT HANDBOOK.

Prepared by Forest Service, U. S. Department of Agriculture. Government Printing Office, Washington. $2.50. 9 x 6; iv + 841 + xxvi; 1937.

This book is not available for general distribution, but may be purchased in bound form from the Superintendent of Documents in Washington, D. C. It is an excellent source for identifications and uses of range plants. The 294 illustrations, which were nearly all made especially for this handbook, are more than adequate, both aesthetically and practically. For purposes of identification the drawings and photographs are in minute detail and fully labeled. The 339 text accounts include nearly 900 range species and vacant spaces are provided for later insertions. The field edition is in loose-leaf form and a convenient symbol sequence is used in place of numeral pagination. Also, the index may be used as a checklist of preferred names for outstanding range plants. Though prepared for government officials whose duties lie in the management of range lands, this handbook should also be in the botanist's library.

FLORIDA WILD FLOWERS. *An Introduction to the Florida Flora. Second Edition.*

By Mary Francis Baker. The Macmillan

Co., New York. $3.50. 7½ x 5; xiii + 245 + 48 plates; 1938.

From the many flowering plants of Florida the author has selected 800 herbs, shrubs, and trees for this edition (enlarged and more profusely illustrated than the earlier one) and in simple terms has indicated their individual characteristics. There is an excellent key in which the plants, with notations on their size and shape, are classified under color headings. The key also acts as an index and refers the reader to the text where, under family headings, further details are given for the identification tentatively established in the key. There is a general alphabetical index in addition to the key.

De l'Influence de divers Cations sur le Croît microbien. *Étude de l'Action a Doses Diverses du Lanthane, du Cérium, du Plomb, du Mercure, de l'Argent, sur la Multiplication du Bacille Pyocyanique. Rôle des Charges Électriques.*

By Lucien Neipp. Masson and Cie, Paris. 90 francs. 10 x 6½; 430 + 5 folding charts; 1937 (paper).

For this important work the author has studied quantitatively the influence of certain cations on the growth of bacterial colonies. After extensive study of the literature and experimentation he concludes that with the exception of silver, which gave an antiseptic action, all the cations utilized (lanthanum, cerium, lead, and mercury) accelerated the growth when given in small dosages but inhibited it when the doses were large. There appears to be no relation between the value of the electric charge and normal bacterial multiplication, nor can the acceleration or inhibition be attributed to a variation in the electrification of the germs. A bibliography includes 528 titles.

The Structure and Development of the Fungi. *Second Edition.*

By H. C. I. Gwynne-Vaughan and B. Barnes. The University Press, Cambridge; The Macmillan Co., New York. $5.50. 8½ x 5⅝; xvi + 449; 1938.

The second edition follows almost exactly the sequence of the contents in the first edition (Q. R. B., vol. 3, no. 2) with the exception that the section *Forms Resembling Fungi*, is placed in a more appropriate position in the later edition. As before, the new edition deals with the morphology and physiology of fungi and with mycological technique. New advances in the investigation of the fungi are included. There are also additional illustrations, though most of the older illustrations from the first edition have been retained. The bibliography is more extensive than that of the former edition.

Gardening Indoors. *The Enjoyment of Living Flowers and Plants the Year Round, and New Opportunities for Home Decoration.*

By F. F. Rockwell and Esther C. Grayson. Line Drawings by Esther C. Grayson and Laurence Blair. The Macmillan Co., New York. $2.50. 8½ x 5¾; xvi + 201 + 39 plates; 1938.

All of the practical and artistic phases of indoor gardening are covered in this book in a thoroughly adequate manner. With the more general use of air conditioning in homes and apartments, and the use of growing plants as integral parts of modern interior decoration, the newer methods suggested by these well-known authors are timely and valuable. The photographs, charts and classifications of plants for seasonal and special purposes, and a well-organized index make this book an excellent example of a handy volume.

Guide to Eastern Ferns.

By Edgar T. Wherry. Illustrated with line drawings by Olive Stoner and Cyrus Feldman. Science Press Printing Co., Lancaster, Pa. $1.00. 6½ x 4; iv + 220; 1937.

The smallness of the book, the sizableness of the print, and the illustrated descriptive identifications of ferns and fern-allies make this a convenient field aid for the region covered—Pennsylvania and New Jersey to Virginia. Besides an alphabet-

ical glossary of technical terms, there are brief notes on suitable soils for fern cultivation and on the growing of ferns from spores.

MORPHOLOGY

THE NATURAL HISTORY OF THE FRILLED SHARK CHLAMYDOSELACHUS ANGUINEUS. *The Bashford Dean Memorial Volume. Archaic Fishes. Article V.*

By Eugene W. Gudger and Bertram G. Smith. American Museum of Natural History, New York. $1.00. 12¼ x 9½; 243–330 + 5 plates; 1933 (paper).

THE ANATOMY OF THE FRILLED SHARK CHLAMYDOSELACHUS ANGUINEUS GARMAN. *The Bashford Dean Memorial Volume. Archaic Fishes. Article VI.*

By Bertram G. Smith. American Museum of Natural History, New York. $3.50. 12¼ x 9½; 331–520 + 7 plates; 1937 (paper).

The phylogenetic position of *Chlamydoselachus* caused much controversy in its day, and while its exact affinities are still in doubt, there is no question that it occupies a primitive position among the vertebrates. Since it was first described by Garman in 1884, there has been little information published about it. One reason for this dearth of material lies in the fact that this shark has rarely been taken outside of Japanese waters. The authors have gathered all known data, using Dr. Dean's notes and available material, and synthesized them into this book.

The second volume is a detailed anatomical description of the frilled shark in which the author has "endeavored to distinguish those features that represent a high degree of differentiation, from others that link *Chlamydoselachus* with the most primitive fishes." The author concludes that the frilled shark "presents a strange assemblage of characters ranging from the very primitive to highly differentiated. *Chlamydoselachus* is a deep-sea adaptation of some rather ancient type of shark, and is now waging a losing battle in the struggle for existence."

Both volumes are excellently prepared and profusely illustrated. This rare and interesting shark has now received some of the attention it so well deserves.

A MONOGRAPH ON VEINS.

By Kenneth J. Franklin. Charles C Thomas, Springfield, Ill. $6.00. 9 x 6; xxii + 410; 1937.

In this book the writer

> has attempted, however imperfectly, to show what is permanent in our knowledge of veins, to indicate where additions of value are even now being made to it, and where opinions about certain aspects of it are changing in the light of fresh research. The review is primarily intended for those engaging in such research, though it should also be of use to others, who wish to know more about the veins and the important parts they play in the harmonious interactions within the living organism. It has been "possible to formulate from a very scattered literature a reasonably consecutive and integrated story. Indeed, nothing has surprised the writer more than the way in which the individual pieces have fitted together."

A work of first rank. The bibliography runs to 27 printed pages and there is an adequate index.

LEITFADEN DER ENTWICKLUNGSGESCHICHTE DES MENSCHEN.

By Horst Boenig. Georg Thieme Verlag, Leipzig. RM. 10.20 (paper); RM. 11.80 (bound); (Outside of Germany, except Palestine, 25 percent less). 10 x 7; x + 266; 1938.

This textbook of human embryology has been very well illustrated both with actual photographs and many diagrams. It is clearly written and includes discussions of the most recent discoveries in the field. The author starts with an account of the development of the sperm and egg, then proceeds to an account of the fertilized ovum, placentation, and finally the development of the various organs. About three-fourths of the book is concerned with organ development.

THE DISSECTION AND STUDY OF THE SHEEP'S BRAIN *as an Introduction to the Study of the Human Brain.*

By James Wilkie. Foreword by T. B.

Johnston. Oxford University Press, New York; Humphrey Milford, London. $2.25. 7¼ x 4¾; xv + 95; 1937.

While this book is primarily concerned with the brain of the sheep the author frequently refers to the human brain for the purposes of comparative anatomy. An excellent guide for pre-medical as well as medical and general students of brain anatomy. It is well illustrated and indexed.

PHYSIOLOGY AND PATHOLOGY

OCCIDENTAL THERAPEUTICS IN THE NETHERLANDS EAST INDIES DURING THREE CENTURIES OF NETHERLANDS SETTLEMENT. (1600–1900).

By D. Schoute. Netherlands Indies Public Health Service, Batavia. 10⅝ x 7½; iv + 214; 1937.

In this book is given, in abbreviated form, the substance of two larger volumes (printed respectively in 1929 and 1935, in Dutch) on the history of medicine and surgery and the development of sanitary measures during three centuries in the East Indian Archipelago. It is a history of the struggles of the United Netherlands East India Company to keep illness and death rates reduced to a minimum during the long voyages around the Cape of Good Hope and of the growth of hospital and medical service in the Dutch East Indies.

We read that in 1621 "De Gouden Leenw made an unheard-of short trip" (4 mos., 4 days) from the Netherlands to Batavia. Death rates aboard ship were frequently appallingly high. The Princesse Royale (sailing in 1647, 212 days at sea, 300 crew) lost 132 of the crew by death and "arrived, nearly all hands sick in berths." Scurvy and foul drinking water or no water at all were the chief worries of the early voyagers. When the drinking water became wormy it sometimes was put through a process of purification "as best as could be done by dropping glowing iron or a big heated cannon-ball into it, mostly a thirty-pounder." Sometimes on a voyage greatly prolonged by adverse winds, when scurvy was particularly bad, the ship would stop at St. Helena or the Cape. With an abundance of fresh vegetables and fruits recovery of passengers and crew seemed little short of miraculous, but more often than not, after a short time again at sea, a new crop of troubles appeared in the form of fevers which were much more to be dreaded than scurvy.

The volume is well documented and contains indexes of personal names and of subjects. Of particular interest to physicians and public health workers will be the sections dealing with the evolution of hospital service in the Dutch East Indies, and the steps which were taken to improve general health conditions. The author includes many interesting mortality tables which he has collected from various authentic sources.

THE FIGHT FOR LIFE.

By Paul de Kruif. Harcourt, Brace and Co., New York. $3.00. 8½ x 5¾; [10] + 342; 1938.

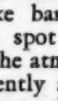
This book continues the appeal for wider and more intelligent application of existing medical knowledge and techniques to humanity in the mass that was begun in the author's preceding volume *Why Keep Them Alive?* It is written in the fine and gallant spirit of a passionate crusader. Righteous indignation bursts forth on nearly every page at society's too calm and placid acceptance of some of the more glaring imperfections of a world that indubitably falls short of complete impeccancy in a number of respects. The principal targets this time include maternal mortality, gonorrhea, syphilis, tuberculosis, and poliomyelitis.

Whether one prefers the warmly emotional or the coldly intellectual technique for diffusing ideas and information is a matter of taste, in respect of which individual human beings differ. But regardless of tastes there surely can be but few persons, if any, who will not find this book fascinatingly interesting. Its deep sincerity is evident, and its scientific reporting is meticulously accurate in detail.

THE PATIENT AND THE WEATHER. *Volume IV, Part 3. Organic Disease. Surgical Problems.*

By William F. Petersen with the assistance of Margaret E. Milliken. Edwards Bros., Ann Arbor, Mich. $10.00. 10¾ x 8¼; xxxvii + 651; 1938.

In this volume the author deals with some acute abdominal and surgical diseases in connection with meteorological influences after having discussed in former volumes this influence on general physiology, pathology and some groups of special diseases, as, for instance, psychosis (cf. Q. R. B., Vol. 10, No. 3, 1935). The surgical diseases of the abdomen like ulcer of the stomach, gall bladder diseases, acute pancreatitis, appendicitis, etc. are considered from the point of view that they represent acute vascular and smooth muscle dysfunction, so that a uniform mechanism underlies them. The author presents for every sort of disease a good many detailed medical histories with meteorographs to illustrate the different clinical events. This casuistic material is of the greatest interest in many directions, but it should be completed by an adequate statistical technique to prove that there is really a coincidence or at least a correlation between the supposed abnormal fluctuations in vasoconstriction and dilatations and some components of the weather, like barometric pressure, humidity or sun spot emanations, etc. The weather, as the atmospheric environment, plays evidently an important rôle in the course of many diseases, but up to the present its influence has not been made as clear as that of such other environmental factors as parasites, diet and social conditions. Perhaps this exhaustive work will show the way for a more exact study.



THE SPAN OF LIFE.

By William M. Malisoff. J. B. Lippincott Co., Philadelphia. $2.50. 8 x 5⅝; 339; 1937.

This treatise professes a profound faith in science, and yet never loses an opportunity of misrepresenting the results of careful, thorough, and honest scientific work. The author starts by discussing length of life and its underlying phenomena, passes to theories of aging, and finally gets on to the prolongation of life. The style throughout is cheap and tawdry wisecracking journalese. Optimism about the future in spite of the present sad state of affairs is professed, and a foundation is proposed to synthesize all that the many branches of science have to offer. Possibilities are even suggested for its financial maintenance. But where are the ones to start Malisoff's new Coöperative Research Foundation for the attainment of astounding age going to come from when all of "the grey little men of science, proud of their techniques and miniature certainties—have moved into a cosy apartment of science and there they will stay, away from the chaos of the pioneering front?" Perhaps the idea is that unscientific pioneers will do the job.

Doubtless a poorer book than this *could* be written on longevity.

WHY GROW OLD? *A Guide-Book for the Man Who Seeks to Remain Physically and Mentally Young.*

By Frank S. Caprio and Owsley Grant. Maxwell Droke, Indianapolis. $2.50. 8¼ x 5½; [18] + 204; 1937.

The past several years have seen a multiplicity of books of the general type that conveys vast encouragement that "life begins at forty" in a real sense, and that if a man takes care of his heart, his stomach, his waistline, his gonads, and his prostate, he can enjoy life a long way past his allotted three score and ten years. The present volume falls exactly into the above mentioned general type, and reveals nothing original with regard to either material or presentation. As a matter of fact a man of the meanest intelligence will find little here that he does not already know, either as a result of trial and error, of reading magazine advertisements, or of exercising a little common sense. Each chapter of the book is summarized in the form of either a question list, or a do and don't list with regard to such topics as diet, exercise, sleep, sex, and mental traits. There is no index.

SYMPOSIUM ON HORMONES. *Sigma Xi Lectures for 1936–37 Ohio State University.*

Various Authors. Ohio Journal of Science, Vol. 37. $1.00. 9¾ x 6¾; 315–463; 1937 (paper).

The following papers are included in this symposium: *The Growth Hormones Found in Plants*, by George S. Avery, Jr.; *Responses of Plants to Hormone-Like Substances*, by P. W. Zimmerman; *Nervous System and Internal Secretion*, by Leon Asher; *The Gastrointestinal Hormones of Autacoids*, by A. C. Ivy; *The Prolongation of Insulin Action*, by C. H. Best; *The Male Sex Hormone —Some Factors Controlling Its Production and Some of Its Effects on the Reproductive Organs*, by Warren O. Nelson; *The Female Sex Hormones*, by Hans O. Haterius; *The Thyroid Hormone*, by David Marine; *Hormones of the Adrenal Gland*, by Frank A. Hartman; *The Hormones of the Anterior Pituitary*, by Oscar Riddle.

None of the authors has attempted to make an exhaustive survey of his subject. Instead each has taken some more or less particular aspect of the selected topic for elucidation and brief discussion. A reference list or bibliography is appended in all cases except in the paper by Asher.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 467. Abt. V, Methoden zum Studium der Funktionen der einzelnen Organe des tierischen Organismus, Teil 10, Heft 7. Allgemeine und vergleichende Physiologie (Ergänzung zu Abt. V, Teil 2).*

Containing following articles: *Lichtquellen für wissenschaftliche Zwecke*, by Marie Wreschner; *Instrumente und Methoden zur elektrischen Strom- und Spannungsmessung*, by Marie Wreschner; *Zell- und Gewebetrennung*, by Martin Behrens; *Fraktionierung und Reindarstellung organischer Substanzen nach dem Prinzip der Tswettschen Adsorptionsmethode*, by Alfred Winterstein.

Urban and Schwarzenberg, Berlin. RM. 13. 10 x 7; 239; 1938 (paper).

The first, a short paper, illustrates various sources of light adequate for scientific work. The second article, occupying about half the pages of this *Lieferung* treats methods of measuring electric current and voltage. In the third, Behrens describes a new method for separating cells and tissues which involves initial freezing of the organs. Winterstein treats the successful utilization of Tswett's chromatographic adsorption method for fractionating and purifying colorless organic substances. This method, first perfected by Tswett in 1906 for coloring matter of plants, was not universally known as the work was published in Russian, but received further impetus at its rediscovery by Kuhn in 1931.

PAVLOV AND HIS SCHOOL. *The Theory of Conditioned Reflexes.*

By Y. P. Frolov. Oxford University Press, New York. $4.00. 8½ x 5½; xix + 291 + 26 plates; 1937.

The method and reasoning by which Pavlov and his school attacked the biological problem of nervous activity through the aid of conditioned reflexes form the basis of this book. The context is made clear enough to be intelligible to the average biologist. Although there is little bibliographical material, the reader can obtain some insight of the character of the great Russian physiologist from the way he conducted his laboratory and by the great reverence he inspired in his co-workers. The translation has been excellently rendered by C. P. Dutt. There are several illustrations and an index.

MEDICAL RECORDS IN THE HOSPITAL.

By Malcolm T. MacEachern. Physicians' Record Co., Chicago. $3.00. 9⅜ x 6½; xv + 374 + 1 folding chart; 1937.

In this book the Associate Director of the American College of Surgeons briefly describes the development of the medical record and the important rôle it plays in clinical and research medicine; presents 99 sample forms for use in collecting the data comprising the record; gives suggestions as to the indexing, filing and storing of histories; and includes a great deal of general information which makes the book a useful manual for the record librarian.

An addendum of 44 pages contains a

list of journals, texts and monographs prepared by the Department of Library Research of the American College of Surgeons to aid in the selection of books for the hospital library. There is a bibliography of 34 titles and an index.

THE BIOLOGY OF PNEUMOCOCCUS. *The Bacteriological, Biochemical, and Immunological Characters and Activities of Diplococcus Pneumoniae.*

By Benjamin White with the collaboration of Elliott S. Robinson and Laverne A. Barnes. The Commonwealth Fund, New York; Oxford University Press, London. $4.50. 9⅜ x 6; xvii + 799 + 10 plates; 1938.

This exhaustive monograph will become indispensable for everyone who is working on the experimental, clinical or epidemiological phases of pneumococcus infection. The classification of pneumococci according to the different types, the knowledge of which is so important for treatment, is given with full details. Especially is all the literature on the bacteriological, biochemical and immunological characters of pneumonococci examined and reviewed. There is a bibliography of 1593 titles, covering more than 100 pages.

LE SYSTÈME NERVEUX VÉGÉTATIF.

By J. Tinel. Masson et Cie, Paris. 160 francs. 10⅛ x 6¼; 847; 1937 (paper).

This comprehensive treatise includes a survey of the physiology, anatomy, and histology of the sympathetic and parasympathetic nervous systems; the action of these systems on the senses, muscular tonus, and trophic reflexes; anatomic and reflex syndromes involving the nervous system in certain diseases; hormonal and pharmaceutical influences; and therapeutic measures, surgical and medical, in neural diseases. A selected bibliography of only the most important or comprehensive works, an index and 603 excellent illustrations are included.

LES ACQUISITIONS NOUVELLES DE L'ENDOCRINOLOGIE. *Troisième Édition entièrement refondue et augmentée.*

By R. Rivoire. Masson et Cie, Paris. 45 francs. 9¾ x 6½; 264; 1937 (paper).

The appearance of a third edition of this work is testimony to its interest and success. The present edition has been entirely rewritten to include the newest findings, and two chapters, one on the thyroid and the other on the thymus and epiphysis have been added. For those not yet acquainted with this book it may be mentioned that it brings together the most important and definitive findings of physiological, biochemical, and clinical research on the endocrines. Unfortunately the work lacks a bibliography or documentation other than the mention of names.

THE HAIR AND SCALP. *A Clinical Study (With a Chapter on Hirsuties). Second Edition.*

By Agnes Savill. William Wood and Co., Baltimore. $4.75. 8½ x 5½; viii + 309; 1937.

Following a summary of the structure and physiology of the hair, the principal diseases of the hair and scalp are arranged on a symptomatic basis. The probable etiology, clinical appearance, and alternative courses of treatment are described. The book is intended primarily for practicing physicians and medical students, but many cautions are directed at hairdressers. Lay people who are willing to wrestle with medical terminology can obtain sound advice on the relation of general bodily health to hair growth. The illustrations are adequate and there is an excellent index.

MACLEOD'S PHYSIOLOGY IN MODERN MEDICINE. *Eighth Edition.*

Edited by Philip Bard, with the collaboration of Henry C. Bazett, George R. Cowgill, Harry Eagle, Chalmers L. Gemmill, Magnus I. Gregersen, Roy G. Hoskins, J. M. D. Olmsted and Carl F. Schmidt. C. V.

Mosby Co., St. Louis. $8.50. 9½ x 6½; xxxv + 1051; 1938.

Due to the many developments in the physiological sciences the distinguished group of physiologists who take part in the revision of this important text have practically rewritten the subject matter. Professor Bard states in the preface that "the present volume makes no pretense of being a text-book of 'applied' physiology." Clinical material, however, has been called upon when it illustrates particularly well fundamental physiological principles. The volume is a well-rounded, authoritative treatise—a worthy successor to the earlier editions. It is abundantly illustrated, extensively documented and has a useful index.

TEXTBOOK OF EXPERIMENTAL SURGERY.

By J. Markowitz. William Wood and Co., Baltimore. $7.00. 9 x 6; xv + 527; 1937.

Experimental surgery, according to Dr. Markowitz, plays too small a part in the training of surgical students. It is by this means that one obtains practice in the fundamental but very essential techniques for later use in the human operating room. The importance of a mastery of physiology by the surgeon is especially stressed. Accompanied by numerous clear illustrations, the text material describes in detail the operative methods for use on various tissues and organ systems of experimental animals, particularly dogs. A fine book for the medical student or clinician.

THE RABBIT TEST *for the Detection of Chorionic Tissue in the Body and the Determination of Its Proliferative Activity.*

By S. B. Anklesaria. With a Foreword by Emil Novak. Popular Book Depot, Bombay H. K. Lewis and Co., London. 12s. 9¾ x 6¾; xvi + 161 + 8 plates + 2 graphs; 1937.

After giving a brief review of female sex hormology (human) and the technic of the original Aschheim Zondek test, the author discusses in considerable detail reproduction and ovulation in the rabbit, advantages of the rabbit test over other biological tests, technic, possible fallacies, clinical significance of the biological pregnancy tests, etc. The volume contains a number of illustrations, a lengthy bibliography and author and subject indexes. Emil Novak contributes a preface.

MAN, BREAD AND DESTINY. *The Story of Man's Food.*

By C. C. Furnas and S. M. Furnas. Williams and Wilkins Co., Baltimore. $3.00. 8½ x 5¾; xix + 364; 1937.

By a judicious mixture of facts, anecdotes, and theories, the authors have succeeded in presenting a book which is an informative and far from dull account of the chemical constituents of food, metabolism, nutrition, diet, simple cooking rules, and food fads. It is well written and superior to the many popular and semi-popular publications on the subject. There is an extensive and adequate bibliography.

RELATION OF STABLE ENVIRONMENT TO MILK PRODUCTION. *U. S. Department of Agriculture. Technical Bulletin No. 591.*

By M. A. R. Kelley and I. W. Rupel. Government Printing Office, Washington. 15 cents. 9¼ x 6; 60; 1937 (paper).

This pamphlet reports the results of a series of tests on selected cows as to the relationship of milk production to environmental conditions such as stables, food, weather and temperature. The effects of stable temperature and temperature changes on milk yield and on physical factors such as respiration are discussed. The problem of water consumption and stable conditions are also considered.

L'ACÉTYLCHOLINE ET L'ADRÉNALINE. *Leur Rôle dans la Transmission de l'Influx Nerveux. Bibliothèque Scientifique Belge, Section Biologique.*

By Z. M. Bacq. Masson et Cie, Paris.

20 francs. 7½ x 5; 114 + 1 folding chart; 1937 (paper).

This book discusses concisely the problems in the chemical transmission of nervous impulses, and the progress so far made in their solution. It is also a valuable contribution to the history of the subject. Dr. Bacq discusses sympathetic, parasympathetic, and motor nerve transmission; the theory and formation of chemical mediators; and comparative nervous physiology.

THE ENDOCRINES IN THEORY AND PRACTICE. *Articles Republished from the British Medical Journal.*

By Various Authors. P. Blakiston's Son and Co., Philadelphia. $3.50. 8 x 5⅜; ix + 278; 1937.

These 28 articles, by outstanding authorities, summing up the solid achievements in the field of endocrinology, will be useful to the student, the general practitioner, and the specialist. They furnish a much needed orientation in a fertile and rapidly developing subject which was in danger of running out of balance. Brief bibliographies are given at the end of each section and there is an adequate index.

COLD SPRING HARBOR SYMPOSIA ON QUANTITATIVE BIOLOGY. *Volume V.*

Edited by Eric Ponder. The Biological Laboratory, Cold Spring Harbor, L.I. $4.50. 10¾ x 7¾; xv + 433; 1937.

The symposia held each summer at Cold Spring Harbor are on some selected aspect of quantitative biology. The subject for the summer of '37 was internal secretions and in this volume are published the 43 papers presented, together with the discussions. Many of the papers are accompanied by figures or tables and necessarily brief reference lists. There is a general index.

THE TRANSMISSION OF DISEASE BY FLIES. *U. S. Treasury Department, Public Health Service. Supplement No. 29 to the Public Health Reports.*

By Cornelius B. Philip. Government Printing Office, Washington. 10 cents. 9¼ x 6; ii + 22 + 3 plates; 1937 (paper).

To bring to the public mind the fact that flies play an important rôle in the transmission of disease is the purpose of this government publication. The house fly is used as an illustration of the order. Sanitary measures for combating these insects and some of the diseases that they carry are discussed.

DIE GESCHLECHTSKRANKHEITEN UND IHRE GEFAHREN FÜR DAS VOLK. *Zweite Auflage.*

By Johannes Breger. R. v. Decker's Verlag, G. Schenck, Berlin. RM. 4. (paper); RM. 5. (cloth). 9¾ x 7; 150; 1937.

A useful description of the damages caused by the venereal diseases. This popular presentation is made for the enlightenment of the people in Germany.

BIOCHEMISTRY

A SHORT HISTORY OF CHEMISTRY.

By J. R. Partington. The Macmillan Co., New York. $2.50. 7¾ x 5¼; xiii + 386; 1937.

A noted scholar in the history of chemistry presents here a valuable account of the progress of his science from earliest times through theories of radioactivity and atomic structure to approximately 1935. The material in this little book is well presented but stated so concisely that its appeal will be limited to students and teachers of elementary courses in the history of chemistry rather than to either laymen or specialists. Relative to its size, it is astonishingly replete with information, although of necessity it treats but lightly of the chemical fields lying within the penumbra of the science, such as physical, biological, industrial chemistry, and neglects details throughout except for the briefest of references. When a contribution is merely mentioned by a phrase in passing or in a listing at the end of a chapter, it would be highly desirable for the author to state a reference to which the reader can turn if his interest impels him in that direction. Only through such assistance can still another brief account of the history of chemistry justify its publication in a field in which

there are already a fairly large number of short works.

Although Partington's book includes more references than do most of its competitors, and can be favorably compared in several respects to such rivals as Hilditch's *Concise History of Chemistry* or Moore's *History of Chemistry*, it nevertheless will probably be considered simply another elementary text. What is needed today is not a repetition of what is already on the market, but rather a comprehensive treatise, similar to von Meyer's great standard work which is no longer procurable, detailing the progress of chemistry in its various branches to the present day.

KATALYSE UND DETERMINISMUS. *Ein Beitrag zur Philosophie der Chemie.*

By Alwin Mittasch. Julius Springer, Berlin. RM. 9.60. 8½ x 5½; ix + 203; 1938 (paper).

The author deplores the fact that for some time chemistry has not been favored with the same attention as physics or biology among philosophers. Here he attempts to remedy this matter, and his specialty, catalysis, is especially suited to the purpose. "Above all," he writes, "one should no longer speak of *Natural Causation* (or determinism) without taking consideration of the definite rôle which catalysis plays in it," and further, that this determinism is no one-sided mechanistic one but that "a broader frame is necessary, in which freedom as well as force can exist." Among the subjects treated are: catalytic causality as a form of stimulus causality, chemical reactions of catalysts, the relation of catalytic causality to other types of causality, the place of catalysts in the order of causalities, and what is included in the concept of determinism. Obviously it is impossible to adequately discuss these points in a short review. The book is well documented, contains a bibliography and is well worth reading by the philosophically-minded scientist.

PRÉCIS DE CHIMIE ORGANIQUE.

By Victor Grignard. Published under the direction of Roger Grignard and Jean Colonge. Preface by G. Urbain. Masson et Cie, Paris. 150 francs (paper); 175 francs (bound). 9¾ x 6½; xiv + 774; 1937.

This excellent textbook, essentially the work of the senior author, who is considered one of the greatest present-day French chemists, has been posthumously published and carefully edited by his son and his collaborator. Although the whole subject of organic chemistry is here treated as a practical rather than an abstract science, the theories of physical chemistry are presented at some length. The methods of degradation are systematized as far as possible and presented for the first time in a French text intended for elementary classes. This work is indexed for subjects and authors. The *Précis* may also serve as an introduction to the more comprehensive fifteen volume *Traité de Chimie Organique*, five volumes of which have already been published.

FOOD VALUES OF PORTIONS COMMONLY SERVED.

Compiled by Anna DeP. Bowes and Charles F. Church. (Obtainable from the Authors, Philadelphia Child Health Society, 311 S. Juniper St., Philadelphia). 50 cents. 10 x 7; [6] + 12; 1937 (paper).

The purpose of this work is, in the words of its authors, "to supply authoritative data on the nutritional values of foods in a form for quick and easy reference." The foods are arranged alphabetically as are also the classes under which the foods are grouped. The system of using portions frequently served as a basis of calculation lends itself very practically to a study of comparative food values and an approximate analysis of diets. Beer lovers who are viewing their disappearing waist lines with well-founded apprehension, happily will be able to find herein proof that a certain much touted dairy health beverage contains 166 calories in comparison with the mere 101 calories of an equivalent amount of lager beer.

FUNDAMENTALS OF BIOCHEMISTRY WITH LABORATORY EXPERIMENTS.

By Carl L. A. Schmidt and Frank W. Allen.

McGraw-Hill Book Co., New York. $3.00. 8 x 5½; xiv + 388; 1938.

This text has for its object "to present sufficient discussion of the subject to give the student a background for his laboratory work. The aim of the laboratory work is to illustrate some of the facts of biochemistry." The book is divided into three parts: (1) a discussion of the facts of biochemistry, (2) laboratory experiments which all the students may carry out during the usual course in biochemistry, and (3) special experiments for groups of two or more students. There is much tabular matter throughout the text, lists of suggested reading and an adequate index.

REPORTS OF THE BIOCHEMICAL RESEARCH FOUNDATION OF THE FRANKLIN INSTITUTE. *Vol. IV, 1936–1937.*

Franklin Institute, Philadelphia. 9¾ x 7; collection of reprints.

A collection of papers on bio-chemistry, histo-chemistry, metabolism of normal and tumor tissues, and five papers on cancer statistics. There is a somewhat pugnacious foreword by the Director.

SEX

A STUDY OF THE GREEK LOVE-NAMES. *Including a Discussion of Paederasty and a Prosopographia. The Johns Hopkins University Studies in Archaeology, No. 23.*

By David M. Robinson and Edward J. Fluck. The Johns Hopkins Press, Baltimore. $3.00. 9¼ x 6; vi + 204; 1937.

The aspect of Greek mores that later ages have found hardest to understand is their attitude towards paederasty. It is true that with a greater understanding of the causal mechanism of homosexuality we are less inclined than our forefathers to condemn it as criminal, but most of us would still regard it as pathological, an unfortunate maladjustment in the developing chain of sex reflexes. Few would echo Plato's eulogy of paederasty. Nevertheless, as the author of this book points out, though it be a faint unintelligibility to see just how essentially related, as cause and effect, were Greek moral-ideals and Greek art, still no progress in interpretive appreciation can ever be made if the layman behaves like an ostrich in the face of something distasteful and ignores its reality. To see only the dazzling shimmer of its reflection in Hellenic art without admitting the strange shadows that, to our sense, so enshroud the sensitive soul of the Greek, is as purblind an occupation as that of separating the known homosexuality of this people from the fine jewel of art it produced in the Athenian Akropolis.

One reflection of the place of paederasty in the Greek mores is the frequent occurrence of the formula: "The boy is beautiful" or "So-and-so is beautiful" on Greek vases. This book catalogues the various names that occur in this formula whether in literature or on vases and suggests identifications of the names with historical characters known from other sources.

About the time of the Peloponnesian War paederasty sank into disrepute, as may be seen by the change in attitude from Plato to Aristophanes. One result of this and of the linked rise in repute of heterosexual love is the more frequent occurrence of nude women in Greek sculpture, whereas before they had been draped and only the men nude. Another is the disappearance of love-names from Greek vases.

SEX, CUSTOM AND PSYCHOPATHOLOGY. *A Study of South African Pagan Natives.*

By B. J. F. Laubscher. Robert M. McBride and Co., New York. $5.00. 9¾ x 6½; xv + 347 + 16 plates; 1938.

This interesting and well written volume —a study of the South Eastern Cape Bantu —is an endeavor to show the influence of the native's beliefs and customs on psychopathology in a primitive culture. In detail Dr. Laubscher, who is on the staff of the Queenstown Mental Hospital, discusses the folklore and mythological characters which play an uncannily real part in the life of the native. His chapter on diviners and sorcerers and the remarkable feats of these men is also one of great interest. However, he fails to explain their superhuman powers attributing them to "psychic sensitivity," "because the word 'intuition' does not cover all its phases." Further chapters describe native

sacrifices, burial and marriage rites, the complicated rituals that involve circumcision and the initiation of the adolescent into manhood. The author's psychiatric interpretation of the various and complex native rites and their bearing on psychoses verge on the Freudian, but his great abundance of factual material, accurately and painstakingly collected, should be of value to the anthropologist as well as entertaining to the layman.

In conclusion the author writes of the native's own conception of mental disorder and its confusion with witchcraft, and devotes two chapters to his native material in the Mental Hospital. He finds that the behavior of the native mental patients is similar to that of European psychotics and suggests that too much emphasis has been laid on the environment in schizophrenia and not enough upon constitutional factors. The final chapter discusses the prevalence of stock theft and homicide and the psychological make-up and culture conducive to these crimes. There is also a chapter on sexual offences.

An appendix supplements the chapter on hospital material containing family histories of some of the native psychotics showing that often members of their families were either witch-doctors or were considered bewitched by their tribesmen. Excellent and interesting photographs accompany the text and there is an adequate index.

DIE STÖRUNGEN DER SEXUALFUNKTION BEI MANN UND WEIB.

By Ludwig Chiavacci. Introduction by Otto Pötzl. Franz Deuticke, Leipzig and Wien. 4.60 marks (paper); 5.40 marks (bound). 9½ x 6⅝; vi + 145; 1938.

This brochure is a mixture of common sense and dubious procedures. The title, which implies a discussion of the disorders of sexual function, is misleading. The book deals mainly with impotence, fifteen pages only are devoted to sterility, and many definite disorders such as nymphomania, satyrism, vaginism, etc. are not touched upon at all. The author sensibly states that frigidity and impotence are not diseases but symptom complexes and for this reason treatment should be varied according to the individual. However, his recommendation that infidelity should be suggested very subtly by the physician in those cases where the impotence of the husband might be due to the frigidity or sexual inadequacy of the wife seems dubious, let alone under-handed. His discussion of treatment for impotence otherwise is an enumeration of patented endocrine preparations, especially the pluriglandular kinds that have so far not found great favor among American endocrinologists. The treatment of sterility is likewise dismissed with a list of various procedures and is of little practical use to the specialist. The book might however serve as a cram survey of the subject to the medical student.

STEP BY STEP IN SEX EDUCATION.

By Edith H. Swift. The Macmillan Co., New York. $2.00. 8 x 5½; xiii + 207; 1938.

In dialogue form this book aims to impart the ideal way of instructing the young in the facts of life. It is not a repetition of the old bee and flower sex education, for Dr. Swift is of the firm opinion that parents should inform their children about "the seamy side of life" before they have a chance to learn it from other and less poetic sources. Her theory may be a sound one, but the manner in which it is portrayed is enough to nauseate the least squeamish. Perhaps the blame may be laid on the bad English and complete artificiality of the author's model characters. At any rate, the whole book leaves one with the impression that these parents spend their entire lives lying in wait to corner their unsuspecting youngsters in order to stuff them to the gills ("casual-like") with sex information, and that before the children are even house-broken they are none too subtly being modelled into ideal mates and parents.

This volume may be of use to some bewildered parents, but it is extremely doubtful that any real children would be

as cooperative as the idealistic son and daughter of this fairy tale.

LOVE AND HAPPINESS. *Intimate Problems of the Modern Woman.*

By I. M. Hotep. With a Prefactory note by Logan Clendening. Alfred A. Knopf, New York. $2.00. 7½ x 5¼; ix + 235 + vii; 1938.

Here is a guidebook for the woman who would seek sexual solace in liaison. The author states that he is not writing for the uneducated girl, because she will never read his words. Yet the person with a moderate degree of intelligence could about as well solve her problem with *Love and Happiness* in a dusky corner of a bookshelf. The folly of preaching morality is here recognized, but not the equal folly of preaching against the repressive hand of the church, social taboos, and political legislation. The material is divided into problems of the girl from 15 to 25, the woman from 26 to 40, the married woman, and the widow.

MENTALITY AND HOMOSEXUALITY.

By Samuel Kahn. Meador Publishing Co., Boston. $3.00. 8 x 5½; 249; 1937.

The homosexuals represented in this study were inmates in the New York Penitentiary for men, and the Women's Workhouse and Correction Hospital for Women. The histories, including the sexual, were obtained by means of personal interviews and supplementary data were provided by the physical examinations and the mental testing. In his general conclusions Dr. Kahn indicates the factors that he considers the most important as concerns the diagnosis, treatment, and prevention of homosexuality. There is no index.

BIOMETRY

RANDOM VARIABLES AND PROBABILITY DISTRIBUTIONS.

By Harald Cramér. The University Press, Cambridge; The Macmillan Co., New York. $2.25. 8⅛ x 5½; [8] + 121; 1937 (paper).

There have been various approaches to the theory of probability. The first of these in point of time supposed that the ways in which an event can happen are divisible into a number of equally probable cases, such as heads and tails in a perfectly balanced coin, and defined the probability as the ratio of the number of favorable cases to the total number of cases. When we, however, broaden the application of the theory of probability to include more than games of chance it is not apparent what are the equally possible cases on which we are to base our probability. The second approach, accordingly, defines the probability of an event as the limit of ν/n as n approaches infinity, where n is the total number of observations and ν is the number of favorable observations. This is the approach followed by von Mises, and in this country by Coolidge. This definition, however, suffers from the defect that the ratio ν/n can be said to have a limit only in a highly Pickwickian sense. Actually as n increases the ratio in general shows a trend towards a limiting value but may exhibit fluctuations that any self-respecting mathematical function would consider beneath its dignity.

Cramér therefore chooses a purely postulational approach. If R is a set of points the probability $P(s)$ of the sub-set S is defined as a non-negative number such that $P(R) = 1$ and $P(S_1 + S_2 + \ldots) = P(S_1) + P(S_2) + \ldots$ whose $S_1, S_2 \ldots$ have no common points. On this basis he develops the mathematical side of the theory, without any reference to the applications.

STATISTICAL METHODS *Applied to Experiments in Agriculture and Biology.*

By George W. Snedecor. Collegiate Press, Ames, Iowa. $3.75. 9 x 6; xiii + 341; 1937.

In this very interesting textbook Professor Snedecor devotes more attention to the application of statistical methods to the interpretation of experiments than do most authors of statistical textbooks. The arrangement of the material is somewhat novel. The student is first intro-

duced to the chi-square method, then to the *t*-test, linear regression, correlation, large sample theory, analysis of variance and covariance, multiple regression and covariance, and curvilinear regression. The numerous examples not only give the student practice in applying the methods, but often illustrate significant points. Thus the examples on chi-square bring out the fact that the same percentage deviations are more significant the larger the populations on which they are based, while the same absolute deviations are less significant. Altogether this is an admirable text with special emphasis on the more recently developed statistical methods.

How to Use Pictorial Statistics.
By Rudolf Modley. Harper and Bros., New York. $3.00. 9¼ x 6⅛; xviii + 170; 1937.

The advantages of graphic presentation of statistical data have long been recognized. However, a bar diagram or other graph, while it shows relations more clearly than a numerical table, is still highly abstract. Somewhat more than a decade ago Dr. Otto Neurath of Vienna began the development of a new form of graph which makes a more vivid and concrete appeal to the layman. This is essentially the division of a bar diagram into units, each of which is represented by a symbol of the subject. Thus a bar representing 15,000,000 people is replaced by fifteen schematized men, each representing a million people. The author, who has had a large share in introducing the method into this country, describes the technique of planning and making such pictorial graphs as well as pictorial diagrams not involving numerical relations. He sees a wide future for the method in schools and museums, and wherever quantitative data are to be presented to the layman. For a trained audience, however, he prefers the older forms of graphs.

Elements of Statistical Method.
By Albert E. Waugh. McGraw-Hill Book Co., New York. $3.50. 9 x 6; xv + 381; 1938.

This textbook differs little from the majority of statistical volumes that have recently appeared. It makes no attempt at specialization in a particular field, but gives a variety of examples, mostly from economics and biology. Development and derivation are largely omitted. Each chapter ending contains a number of exercises, designed primarily to evoke thought on the fundamental principles rather than on arithmetic methods. A series of appendices is included, giving short tables and a list of statistical books.

PSYCHOLOGY AND BEHAVIOR

The Mind of the Juror *as Judge of the Facts or the Layman's View of the Law.*
By Albert S. Osborn. Introduction by John H. Wigmore. Boyd Printing Company, Albany. (Also obtainable from the author 233 Broadway, New York). Library Edition $4.50; Students Edition $3.50. 9⅜ x 6⅛; xv + 239; 1937.

For nearly forty years Mr. Osborn has been an expert witness on documents before courts in nearly every state in the Union. It is natural that in this long service a shrewd observer should have accumulated a valuable fund of first hand knowledge on how our courts are actually run. In this book he presents his observations together with suggestions for the more efficient administration of justice. These include greater care in the selection of an intelligent type of jurors, abolition of the requirement of a unanimous verdict, appointment of judges for long terms instead of their election for short terms, repeal of the statutes which in many states gag the judge and leave the jury at the mercy of the sophistries of the contending lawyers, and exclusion from the bar, so far as possible, of those attorneys whose only interest in their profession is the amount of money they can acquire.

When a man of Mr. Osborn's essentially conservative temper and with his wide experience of our courts presents such an unflattering picture of their actual operation we may well pause and think. What

stands in the way of such reforms as he proposes?

The answer is easy and short and offensive; the legal profession in this country stands in the way. Next week steps toward this reform could begin, next month reform could be extended, and next year and the years following it could be completed if a majority of the legal profession earnestly desired reform.

The opponents of this proposed means of making the administration of the law more efficient by increasing the powers of the judge are the present day successors of those who originally took this power away. They are the advocates who strenuously object to any interference with their law practice by a man sitting on a bench as a judge. They say this frankly. One of these opponents in a burst of confidence said, 'It is difficult enough to defeat my opponent without also being obliged to defeat the judge.'

Every person interested in the more efficient administration of justice, whether lawyer or layman, should read and ponder this book.

A HERD OF RED DEER. *A Study in Animal Behaviour.*

By F. Fraser Darling. Oxford University Press, New York; Humphrey Milford, London. $5.50. $8\frac{3}{4}$ x $5\frac{1}{2}$; x + 215 + 8 plates + 2 folding maps; 1937.

Most of life history studies have been among insects, birds, and a few other representatives of our animal life but the mammals have been sadly neglected, mainly because it requires a lot of time to observe their entire life history and because their size makes it infeasible to place them in cages for experimental purposes. By overcoming these difficulties, Darling has done a notable piece of work.

This book tries to give the plain tale of an animal's life, of the things it does and is trying to do, of its relations with its fellows and with men, and of the things to which, as long observation leads me to believe, it responds. The study can be considered in no way complete, for now, after two years, I am left with more problems to solve than I knew of at the outset.

The author is too modest when he says, "this book tries. . . ." For two years he stalked the deer of northwestern Scotland in all kinds of weather, and the result is this entertaining, penetrating, and scientifically important story of their doings. He found that the deer spend their lives in limited areas, that the hinds and stags have their own territories according to the seasons, that the stag's antlers are used as an erotic zone. There is also a wealth of material on movement, insect parasites, territory and population, reproduction, etc.

The observations are very honest and the author studiously avoids placing any anthropomorphic interpretations on behavior although he admits that the objections to anthropomorphism can be carried too far. Mammalogists, students of animal behavior, naturalists, deer raisers, and even deer hunters, will find this book absorbing and valuable. A notable contribution.

THE MEASUREMENT OF OUTCOMES OF PHYSICAL EDUCATION FOR COLLEGE WOMEN. *By Elizabeth Graybeal. University of Minnesota Press, Minneapolis.* $1.00. $9\frac{1}{4}$ x $6\frac{1}{4}$; viii + 80; 1937 (paper).

A controlled experiment was set up in which half of an entering class at the University of Minnesota participated in the regular required courses in physical education and the other half did not. The problem was to ascertain the differences, after a period of examination, in the outcome of four objective tests; attitudes, knowledge, motor ability, and posture. This pamphlet gives a detailed description of these tests and their outcome and many tables present the various scores of the two groups. It was found that those who participated in the prescribed courses demonstrated an improvement in the functions tested. While this was not always great, it was demonstrated so consistently that the author concludes that a college requirement in physical education would seem justifiable. A bibliography and appendix supplement the text.

L'INTELLIGENCE DES ANIMAUX. *Insectes sociaux, Chevaux mathématiciens et causeurs. By Maurice Lecat. Librairie Castaigne,*

Brussels. 3 francs. 9¾ x 6⅛; 22; 1937 (paper).

This paper represents a chapter from the larger work of M. Lecat entitled *Le Maeterlinckianisme*, and deals with the philosopher's ideas on the intelligence of animals. The text is well documented with quotations from Maeterlinck's work, and is intended to give us some idea of the man and his work. The author very carefully refrains from making comments that would in any way influence the reader's opinions either of the man or of the value of his contributions. The strain upon the imagination in trying to visualize an ant that . . . "lives only for his God . . .", or a horse that can calculate the fourth root of 7,890,491 is so great that we are inclined to agree with other critics in saying that Maeterlinck was a mystic, and that some of his work would hardly withstand the test of scientific criticism.

THE INTERRELATIONSHIP OF DRIVES IN THE MALE ALBINO RAT. *II. Intercorrelations Between 47 Measures of Drives and of Learning. Comparative Psychology Monographs, Volume 14, Number 6, Serial Number 72.*

By E. E. Anderson. Johns Hopkins Press, Baltimore. $1.50. 10 x 6¾; 119; 1938 (paper).

Because in the earlier study the different drives were measured by a comparatively limited number of tests, this study in which 47 measures were obtained was undertaken to get more comprehensive data concerning the interrelationship of various measures to learning performance. The monograph gives a detailed description of the various tests and many tables illustrate the scores. In general the author found that the correlations of the earlier and less extensive study were higher than those of the present one, but there were many cases of essential agreement between the two. He believes that the inadequate preliminary adaptation and the lower degree of motivation for the test in the earlier experiment account for the discrepancies between the two studies.

A BIOLOGICAL APPROACH TO THE PROBLEM OF ABNORMAL BEHAVIOR.

By Milton Harrington. Science Press Printing Co., Lancaster, Pa. $4.00. 9 x 6, 459; 1938.

Credit must be given to Harrington for following an earlier destructive criticism of the Freudian theory by this attempt at constructing a different angle of approach to the problem. The theory advocated here, that all abnormal behavior results from anatomical and physiological inadequacy of the individual, is obviously not an entirely new one. Perhaps somewhat insufficient acknowledgment is extended to others with a similar point of view. The author presents his arguments in a logical manner, but does not hesitate to recognize weaknesses in his purely mechanistic theory. The work deals with "psycho-physiology" or the mechanism of behavior, and "psycho-pathology" or the abnormal behavior which arises from this mechanism. Treatment of abnormality is left to a further volume.

THE PSYCHOLOGY OF EARLY GROWTH *Including Norms of Infant Behavior and a Method of Genetic Analysis.*

By Arnold Gesell and Helen Thompson, Assisted by Catherine S. Amatruda. The Macmillan Co., New York. $4.00. 10 x 7½; ix + 290 + 15 plates; 1938.

This volume presents to a certain degree a continuation and elaboration of a former publication *The Mental Growth of the Preschool Child* that is now out of print. It deals especially with norms of infant behavior as to mental development in early childhood but includes also growth and anthropometric examinations. It is subdivided into three parts: I. Methods and procedures; II. Norms of infant growth; III. Analytical appraisal of infant growth status. As in the former investigations of the authors, it gives a great many different results of high value with regard to the mental development in infancy. These results are important for the study of child hygiene and individual psychology. For an anthropometric average the number of 107 infants studied and distributed among

different age groups may not be large enough.

DIFFERENTIAL PSYCHOLOGY. *Individual and Group Differences in Behavior.*

By Anne Anastasi. The Macmillan Co., New York. $2.75. 7⅞ x 5¼; xvii + 615; 1937.

In this textbook, intended especially for the college student, the author has brought together and critically examined the facts and theories relating to variation in psychologic manifestations. In the first part Anastasi introduces the subject with an elementary exposition of the concept and measurement of variability and then proceeds to a discussion of the relation of psychologic differences to heredity, environment and physical constitution. In the second part the subject matter concerns subnormal intellect, genius, and the association of sex, race, and urbanism with certain mental characteristics. The soundness of the author's views and the adequate bibliography serve to make this an excellent work for its purpose.

EMBRYONIC MOTILITY AND SENSITIVITY. *Monographs of the Society for Research in Child Development. Volume II, No. 6 (Serial No. 13).*

By W. Preyer. Translated from the Original German of "Specielle Physiologie des Embryo" by G. E. Coghill and Wolfram K. Legner. Society for Research in Child Development, National Research Council, Washington, D. C. $1.00. 9 x 6; v + 115; 1937 (paper).

An important translation. The original (of which this is only a part, beginning with Chapt. 6) appeared in 1885 and "summarizes the earlier literature on the physiology of the embryo with Preyer's critical consideration and includes Preyer's own unsurpassed observations on embryos." With the present development of interest in genetic psychology and the growing awareness that behavior begins before birth, Preyer's monograph returns again to a well-deserved place on biological book-shelves. The translation is so arranged as to facilitate comparison with the original. The bibliography of over 500 titles is included and there is an index of authors.

PERSONALITY *in Formation and Action.*

By William Healy. W. W. Norton and Co., New York. $2.00. 8 x 5½; 204; 1938.

The author is the director of the Judge Baker Guidance Center, Boston, and this account is based upon 30 years experience with a great variety of personality problems. Psychological theories do not rate a great deal of space, and make way for the common sense of experience and the insight that is presumed to follow upon the heels of the latter. While we agree that it is the common sense that gives to the book what value it has, we are not so sure that the insight is of the sort that goes very far beneath the surface.

THE BACKWARD CHILD.

By Cyril Burt. D. Appleton-Century Co., New York. $5.00. 8½ x 5½; xx + 694; 1937.

A comprehensive discussion of the interrelations of the factors determining backwardness, physical and mental defects, poor environment, etc. The analysis of the sensory and motor defects is based upon a large body of experimental evidence, particularly so in the cases of speech defects and left-handedness. Considerations of general intelligence, temperament, character, and environment figure prominently in the discussions on the diagnosis and treatment of backwardness. There is an appendix devoted to the curves of growth, and another to the statistical criteria.

THE QUEST OF THE OVERSELF.

By Paul Brunton. E. P. Dutton and Co., New York. $3.00. 8½ x 5⅞; 304; 1938.

According to the author this book is an explanation of a system of Yoga altered and adapted to suit modern Western needs.

To the reviewer a system of breathing exercises as a means of attaining spiritual truth has always seemed slightly grotesque. In a quotation from Carrel there is an amusing error: "It is known that certain animals such as the small anthropoid, tardigradum, stop their metabolism when they are dried." Surely for "anthropoid" we should read "arthropod."

THE MANAGEMENT OF EARLY INFANCY. PUBERTY AND ADOLESCENCE. THE PSYCHOLOGICAL APPROACH. THE NEUROTIC CHARACTER. *Individual Psychology Medical Pamphlets—No. 18.*

By E. Joyce Partridge, H. Crichton-Miller, T. A. Ross and F. G. Crookshank. C. W. Daniel Co., London. 2s. 6d. net. 8½ x 5½; 64; 1937 (paper).

The four papers presented in this pamphlet, by psychologists of standing, formed part of a symposium on Mental Health in Childhood and Adolescence. They were originally delivered before the Medical Society of Individual Psychology, London.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

DE VERITATE.

By Edward, Lord Herbert of Cherbury. Translated with an Introduction by Meyrick H. Carré. J. W. Arrowsmith, Bristol, England. 12s. 6d. net. 8½ x 5½; 334; 1937.

This is a translation from Herbert's rather crabbed Latin of the first purely metaphysical work written by an Englishman. Partly because of the difficulties of his style and partly because the main current of English metaphysics has been empiricist, Herbert has been little more than a name to most English students of philosophy. As Mr. Carré points out in his introduction, Herbert in his emphasis on Natural Instinct and its product, the Common Nations, is a precursor of Kant and his *a priori* forms of knowledge. Indeed except for Reid and his followers, Herbert's affinities are with the Continental rather than the British metaphysicians.

In placing at the centre of his scheme of knowledge self-evident judgments of reason, Herbert stands sharply contrasted from the empirical method powerfully advocated by his great contemporary. But he penetrates far more deeply into the metaphysics of knowledge than Bacon, examining assumptions which the latter takes for granted. For the first time in English thought—and it is not easy to find a parallel elsewhere in Europe—a systematic and comprehensive enquiry into the complex processes of knowing is undertaken. That the analysis is often confused and superficial does not detract from the merit of the insight which is achieved. Even in the psychological portions of the work there is, as we have noted, remarkable foreshadowing of later theories. But in its central conception of the organic relation between the impulse of Nature and human thought, in its theory of the reciprocity between objects and the understanding, and in its discussions of the *a priori* foundations of knowledge, it expresses philosophical ideas which look beyond the whole course of English empiricism, by which for nearly two centuries they were submerged.

MAKING PICTURES WITH THE MINIATURE CAMERA. *A Working Manual.*

By Jacob Deschin. Whittlesey House, McGraw-Hill Book Co., New York. $3.00. 9 x 6; xii + 156 + 47 plates; 1937.

For the amateur or even the professional miniature photographer who is seeking exact and detailed directions in every phase of his art this little volume is of extreme value. Mr. Deschin writes as one with experience not only in the art of photography, but also in the field of editing. His editorial work has been seen in several journals in the form of keen and understandable articles on every phase of his work. The book is divided into four parts, namely: (1) Taking the Picture, which includes a discussion of techniques in "shooting" every conceivable type of picture; (2) Making the Negative, which contains all the salient points on equipment and technique in developing; (3) Making the Picture, which gives all the details of printing, enlarging, and touching up the picture; and (4) A Miniature Camera Gallery of 47 plates characteristic of the type of photography displayed in the countless picture magazines found on all our corner news stands. The first three sections are well illustrated by diagrams and line drawings. The volume is completed with a glossary of photographic terms and an index.

MINIATURE PHOTOGRAPHY *From One Amateur to Another.*

By Richard L. Simon. Introduction by Laurence Stallings. Simon and Schuster, New York. $1.75. 7¾ x 5½; x + 168 + 30 plates; 1937.

This little guide to miniature photography has been written to supply the solutions to some of the many problems and questions that confront the candid cameraman as he pursues his hobby along ever-widening trails. Though we could hardly say that the work was the product of a literary genius, it is nevertheless clearly and interestingly written, and spiced up a bit by an occasional photograph illustrating good or bad technique in some phase of the making of the picture. The content is made up of a discussion of such topics as type of equipment, expenses, exposing, developing and printing of pictures. Included also are some notes on publications and gadgets, appendices A and B on how to work several types of cameras, and an index.

SCIENCE IN OUR LIVES.

By Benjamin C. Gruenberg and Samuel P. Unzicker. World Book Co., Yonkers-on-Hudson, N. Y. $1.76. 7½ x 5⅜; xiv + 750; 1938.

A praiseworthy comprehensive study of general science. Simple vocabulary, uncomplicated sentences, and concrete, familiar examples make this a practical, teachable textbook. Herein all science is presented as a related picture, and the social significance of scientific achievement is stressed. The contents are divided into two parts each of which is subdivided into four units. Air, water, fire, and earth are the four "elements" developed in Part I. Work, electricity, life in respect to nutrition, environment, reproduction, heredity, sound and hearing, light and sight, and reflexes and instincts are dealt with in Part II as part of man's control over nature. There are also 97 problems to stimulate the natural curiosity of the student and over 300 described activities for practical demonstrations of scientific procedure.

BOOKS AND DOCUMENTS. *Dating, Permanence and Preservation.*

By Julius Grant. Chemical Publishing Co., New York. $4.50. 8½ x 5½; xii + 218 + 12 plates; 1937.

Part I deals with the origin and development of paper from the point of view of dating characteristics and describes tests which indicate the presence or absence of these characteristics. There is a special chapter on the use of fluorescence in ultraviolet light. Part II deals exhaustively with the durability, permanence and preservation of books and documents. The volume will interest librarians, antiquarians and collectors, and all those having anything to do with manufacturing books and documents.

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